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THE INERTIA OF ENERGY[1]

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RELATIVITY may stay or go; the quantum theory may quarrel with the undulatory theory until there is no more left of either of them than of the traditional Kilkenny cats; and the unscientific may scoff: "What are the latest conclusions of science? I have not seen the morning papers." Yet I think we may safely say that the twentieth century, young as it is, has made at least one permanent contribution of the first magnitude to physical science—the doctrine of the inertia of energy.

To call any concept of physics permanent in these iconoclastic days is perhaps unsafe; yet the case for the inertia of energy is a strong one. Radical though it may be, and subversive of established ideas, it comes nevertheless of an old and respected family. Because Einstein's name is connected with it, it is perhaps rather generally supposed that this doctrine is in some abstruse way a corollary of the theory of relativity and consequently to be regarded with suspicion by the conservative. Not so; nothing has a better right to the name classical. It traces its descent in direct line from Maxwell and Newton; its pedigree is unimpeachable; its arms display no bar sinister. If the theory of relativity also leads to this doctrine, so much the better for relativity; it gains strength rather than imparts it.

The eighteenth century, like all its predecessors, was materialistic in its attitude toward natural phenomena. The modern concept of energy was not recognized; forces of all kinds were regarded as properties of matter, just as gravitational force was regarded until Einstein declared it to be not a material property at all, but

[1] Published by permission of the director of the Bureau of Standards of the U. S. Department of Commerce.

a space property. By the introduction of the concept of energy and its elevation to a rank coequal with matter the nineteenth century made a notable departure from this traditional materialism. At the close of the century the two concepts, matter and energy, divided the province of physical science equally between them.

This joint sovereignty presented to the philosophic onlooker several curious features. In the first place, it was a coalition uniting views as extreme as any in the history of human government, for matter is certainly "material," and energy nothing if not immaterial. Moreover, matter had an established position with a pedigree of centuries behind it; it had been recognized "always, everywhere and by all," while energy, when it first came into notice, had even been introduced as a state or condition of matter. Its enthronement as joint sovereign had come about by virtue of its executive ability, the power it had shown of correlating phenomena and reducing the hitherto independent and intractable to law and order. Similar ability was shown by Mexico's benevolent despot, President Diaz, when he enlisted the roving bandits as members of the rural police force. The conservative citizens of the domain of physics, while acknowledging the equal sovereignty of energy, always retained in their hearts a special feeling of respect for matter as the ultimate reality, the substance of things, whose existence permitted energy to be, and without which energy would be but an empty name.

This state of mind was rudely disturbed when Einstein announced that henceforth the tail was to wag the dog; that matter must be regarded merely as another aspect of that protean concept, energy; that there was a definite numerical equivalent relation between them. Just as 4.2×10^7 ergs of energy equal one calory of heat, so one gram of matter may disappear as such, giving rise to 9×10^{20} ergs of energy.

But how can matter disappear? What then becomes of the law of conservation of matter, established over a century ago by Lavoisier, and long regarded as a great and permanent contribution to science? And how can energy appear without a corresponding disappearance of energy elsewhere? What of the law of conservation of energy, which has, since its foundation, enjoyed an esteem equal to that accorded the law of conservation of matter?

The doctrine of inertia of energy declares unflinchingly that both laws are wrong; that matter may actually disappear as such and energy in equivalent quantity appear in its stead. In place of the two former laws we have one broader principle—the conservation of matter-energy.

But under what circumstances does matter disappear, and why has this strange fact never been shown by the many careful and ingenious experiments on gravitation carried out during the nineteenth century? The explanation lies in the very large coefficient in the relation between matter and energy, 9×10^{20}. Experiment is well-nigh hopeless before the twentieth power of ten. The coefficient for the mechanical equivalent of heat contains only the seventh power of ten, and this permits an experimental verification of the principle. This fact undoubtedly assisted the physicists of the mid-Victorian period in familiarizing themselves with the idea that work and heat were interconvertible—a concept as strange to the physicists of those days as the equivalence of matter and energy is to us of to-day. It is said that Poggendorff refused to publish Mayer's paper on the mechanical equivalent of heat. "Why," said he to the author, "if this be true, water could be warmed by shaking it!" To this Mayer for some time could find no reply. The answer came only when it was shown experimentally that such was indeed the case. There is no denying the difficulty of a concept as revolutionary as the annihilation of matter and the creation of energy; and unfortunately we can not verify the theoretical principle by experiment. This theory asserts that when a hot body cools off, emitting heat and light, it must lose a little of its mass. For example, a gram of water at 100° will have when cooled to zero a mass less than one gram by the mass-equivalent of the energy that has been radiated away. To calculate this we divide the 100 calories, or rather 4.2×10^9 ergs, by 9×10^{20}, obtaining about 5×10^{-12} gram. Now even when dealing with masses of the order of a kilogram it is not possible at present to detect a difference less than one part in a billion (10^9).

The most vigorous chemical reaction known is that of the union of oxygen and hydrogen. In the formation of 18 grams of water about 69,000 calories or 3×10^{12} ergs of energy are liberated. This, on division by 9×10^{20}, gives us for the decrease in mass 3×10^{-9} gram, about one part in six billion.

In the case of energy liberated by radioactive bodies experiment is, at first sight, not quite so hopeless. One gram of radium in transforming into radium D (the first considerable stop-over in the series) would liberate about 130 calories per hour. This transformation is very slow, the average life of a radium atom being 2,600 years, or about 2×10^7 hours. Hence the total energy liberated in the transformation of one gram of radium into radium D (and helium) will be about $130 \times 2 \times 10^7 \times 4.2 \times 10^7 = 1.1 \times 10^{17}$ ergs. Dividing by 9×10^{20} we obtain 1.2×10^{-4} gram, or about one part in 10,000.

But such an experiment is impracticable. Starting with a gram of radium, the total amount transformed in one year would be 0.4 mg, and the actual loss in weight (of radium and helium together) only 5×10^{-8} gram. And to ensure that there is no error introduced by leakage (helium in the form of alpha rays) the containing case of lead would have to be constructed with preposterously thick walls, reducing the proportional change of weight far below the detectable limit.

Passing to the astronomer's laboratory we obtain quantities which seem large enough indeed to measure. The total energy radiated by our sun per second is enormous. Converted into its mass equivalent it gives the rather surprising figure of 4 million tons per second. This is not so easy to detect as might appear, for so super-enormous is the sun's mass that he is good for this rate of expenditure for something like 10 million million years.

So it appears that our sun and all the other stars in the heavens are slowly dissolving into light. Strange and novel as this idea may appear, it is no new thing, for a strikingly similar doctrine was taught in the eighteenth century, based upon the then current materialistic corpuscular theory of light. The following quotation from Nicholson's "Natural Philosophy" (London: 1786) illustrates this point and incidentally shows to what heights of speculation men dared to go in those days.

> If the comets be habitable, they must be possessed by creatures very different from any we have been used to behold and consider. There may, however, be other uses for which it is conceivable that they may have been formed. The matter which composes their tails must fall in process of time to the sun or the nearest planet that may pass through it, where it may supply defects and answer purposes which our total ignorance of its properties scarcely allows us even to conjecture. In the sun it may serve to recruit the waste of matter that luminary may suffer by the constant emission of particles of light.

Perhaps the only distinction to be drawn between eighteenth and twentieth century ideas regarding the decay of the sun's mass is that the eighteenth century idea was thoroughly materialistic, while that of the twentieth century is just the opposite.

It is at once evident that the eighteenth century idea in this matter is properly to be described as Newtonian, for that great philosopher was one of the principal supporters of the corpuscular theory of light; but in what way are we justified in saying that the modern doctrine of the equivalence of matter and energy can be traced back to Newton and to Maxwell?

The principle of the inertia of energy was first announced by Einstein in 1905[2] as a consequence of the special theory of rela-

[2] *Annalen der Physik,* Vol. 18, p. 639, 1905.

tivity. Very soon after[3] he showed that this principle could be deduced from a strictly classical basis. Consider a hollow cylinder with closed ends, containing a movable plug or piston. Suppose at first that this piston is in contact with the left end of the cylinder with a trace of some explosive between them. If this explosive be set off the piston will be driven to the right and the cylinder, by the reaction, to the left. This relative motion will continue until the piston strikes the right end of the cylinder.

To an outside observer, unaware of the presence of the piston within, it would appear that the cylinder, without the application of any outside force, shifted its center of mass (or inertia) slightly to the left, in defiance of classical mechanics. If he was convinced of the correctness of the usually received mechanical principles, he might be led to infer that a concealed mass on the inside of the cylinder had shifted its center of inertia to the right to an extent sufficient to equalize the motion of the cylinder, and the hidden mechanism of the trick would stand revealed to the eye of reason.

Einstein considers a similar cylinder without any piston, but with the left end a little warmer than the right. If a pulse of radiant energy leaves the left end and travels through the cylinder to the right end we have a state of affairs analogous to that of the moving piston. As shown by Maxwell, on strictly classical grounds, radiant energy possesses momentum and will exert a pressure upon a surface against which it strikes; and by Newton's third law of motion, it must exert an equal and opposite pressure upon the surface which it leaves. The effect of this moving piston of radiant energy will be therefore to shift the center of mass (inertia) of the cylinder to the left by an amount too small indeed to be experimentally verified, but which an acceptance of classical theory requires us to recognize. By the cooling of the left end of the cylinder and the warming of the right end a certain amount of energy has been transferred from one end to the other; and to preserve the classical doctrine of the unchangeable center of inertia of a conservative system we must assume the simultaneous transfer of a small inertia from one end to the other. Maxwell showed that radiant energy possessed momentum; to this Einstein added the possession of inertia. In order to preserve unchanged the laws of classical mechanics the inertia equivalent of the energy-piston must be 9×10^{20} ergs per gram. This coefficient is the square of the speed of travel of radiant energy and gets into the formula because the speed of travel of the energy-piston is a factor in determining the shift of the cylinder. Were this speed infinite the cylinder

[3] *Annalen der Physik*, Vol. 20, p. 627, 1906.

would not have time to move at all before the impact on its far end stopped its motion; and the more slowly the energy travels the greater its inertia equivalent. The parallel to the material piston holds throughout.

Mass may be measured either by its inertia or its weight; in fact, inertia and weight (or gravitation) have always been regarded as the only two properties of matter sufficiently characteristic to serve as a basis for its definition: matter is that which possesses inertia and exhibits gravitation. It was the failure to show any ability to gravitate which brought the abandonment of Kelvin's ether-vortex atoms; inertia they had in plenty. Does energy possess weight as well as inertia?

We have seen that in the case of radioactive bodies there is a loss of energy which, in several thousand years, should cause a measurable change in inertia. There is no doubt that radioactive products of the necessary age lie ready to hand in the form of uranium and lead, the beginning and end of a chain of transformations which has required many thousand years for completion. So slowly does uranium break down that a portion of it may travel the long way to lead, while another portion still remains as uranium. If during these transformations the escaping energy carried off inertia without weight we might expect that uranium and lead would have equal weights but different inertias, and in consequence would not exhibit the same acceleration under the action of gravity. But this question of the proportionality of weight and inertia, or the variability of gravitation with the nature of the substance, has been subjected to very searching experimental tests, the most delicate of which are those carried out by Eötvös with his torsion balance.[4]

For most substances this investigator found that inertia and weight were proportional to an accuracy of one part in 200 million; for radium compounds, where only comparatively small quantities were available, the precision reached was about two parts in a million.

We may therefore safely conclude that energy possesses both of the characteristic attributes of matter, and that matter may be converted into energy with a definite numerical equivalent relation.

It is a poor rule that does not work both ways. If the union of oxygen and hydrogen to form water results in a slight diminution in the mass of the reacting substances, how will it be in the case of electrolysis of water? Will the resulting oxy-hydrogen gas weigh a trifle more than the water?

[4] *Annalen der Physik*, Vol. 68, pp. 11–66, 1922.

Yes, we must admit this to be the case, though the magnitude of the change is too small for us to pick up experimentally. The increase in mass must measure the energy applied to dissociate the compound. This leads us to view in a new light our concept of potential energy, which ceases to be an imponderable, and becomes a definite weighable quantity.

The idea of matter turning into energy is of such a transcendental character as to cause dismay and confusion to those of us who learned our elementary physics before the discovery of X-rays. Can we form any mental picture which will be helpful?

I think that this is possible. Einstein's theory of gravitation supplies us with a mental picture of matter which lends itself excellently to illustrating the conversion of matter into energy.

Einstein's theory of gravitation stands apart from all other attempts to explain this mystifying phenomenon in that he begins by denying that there is any force of attraction between two gravitating bodies. His strategy is excellent; having denied the existence of such a force he does not have to set up machinery to account for it. He replaces action at a distance by action in contact, of a transcendental nature, perhaps, but one of which a fair analogy can be given. It is like the deflection of a moving object by a surface of constraint.

Imagine a level surface of still water of indefinite extent; this surface will be two-dimensional, having length and breadth, but no thickness. The surface being perfectly flat, the geometry of figures traced upon it will be Euclidean, that is to say, the sum of the angles of a triangle will be exactly 180°, and through a given point only one parallel can be drawn to a given straight line. But suppose the surface, instead of being flat, is spherical, like the surface of the ocean viewed on the large scale; the geometry of figures traced on such a surface will then differ importantly from that of figures on a flat surface. On a spherical surface we can not, of course, draw a straight line in the usual meaning of that term; but we can draw one after Euclid's definition: the shortest distance between two points; and, as every navigator knows, this will be an arc of a great circle. There is a name used in general for such a shortest line traced on a curved surface of any kind: it is called a geodesic line. Its actual shape will, of course, depend on the way the surface is curved and the direction in which the line is drawn. On a cylinder, for instance, a geodesic may be a straight line an arc of a circle or some intermediate form, according as it is drawn parallel, perpendicular or oblique to the axis of the cylinder.

On our spherical surface the three angles of a triangle (con-

structed of geodesics) will exceed 180° by an amount proportional to the area of the triangle. And upon such a surface two arcs of great circles will always intersect each other if sufficiently produced; that is to say, through a given point no geodesic (or "straight") line can be drawn parallel to (that is, not meeting) a given geodesic. A surface possessing these geometrical properties is called a surface of positive curvature.

On such a water-surface a floating particle, if set in motion, and free from the action of all forces, frictional, attractional or otherwise, would travel by the shortest, "straightest" path it could find, obeying Newton's first law of motion with the added condition of being confined to the spherical surface; that is to say, on a curved surface, the natural path of a body moving under the action of no force is a geodesic.

Surfaces of negative curvature may be constructed, on which the geometry is just the opposite of that on a surface of positive curvature; for on such a negatively curved surface the three angles of a triangle sum up to less than 180°, and through a given point more than one geodesic can be drawn parallel to (*i.e.*, never meeting) a given geodesic. Examples of such surfaces are the stem of a wine glass, a saddle or a mountain pass. On such a surface the geodesic, from a Euclidean point of view, would be a curiously twisted line.

Returning now to our flat surface of water, let us render it non-Euclidean by curving it in still another fashion. By careful manipulation it is possible to lay upon the surface of the water a particle of a heavy body such as lead, or even gold, so that it will float. The only thing necessary is to avoid breaking through the surface. The particle then lies supported by the unbroken water surface bent into a cusp or depression. Here we have a surface, normally two dimensional, bent or depressed slightly in the direction of a third dimension in the vicinity of a particle of matter. If we examine the geometry of figures traced upon the curved portion of the water surface, we shall find it non-Euclidean, and of negative curvature. The geodesic of this part of the surface will be a curved line of some kind; but if continued well beyond the cusp in either direction the geodesic will soon be indistinguishable from an ordinary straight line, and the geometry of these distant portions of the surface will be Euclidean.

Suppose now a comparatively heavy particle thus floating and forming a rather deep and widely extended cusp. At a great distance, in a Euclidean region of the surface, suppose a much smaller and lighter particle, which hardly produces any cusp, moving freely

along the surface in a direction that will carry it past the heavy particle at a short distance, well within the latter's cusp. The path of the moving particle, at first a straight line, will as it enters the cusp gradually assume the curved or geodesic form proper to the space in which it finds itself. Assuming no attractive force to exist between the particles, the moving particle will pass on and out of the cusp, its path again becoming straight; but on account of the brief twist to which it was subjected in passing through the cusp the final straight portion of the path will not in general be a continuation of the first straight portion. The particle will have suffered a permanent deflection.

An observer watching the motion of the particle through what we may call Euclidean-Newtonian spectacles, which do not show him the curvature of the water surface, will say: "Yes, on passing the heavy particle the light particle seems to have suffered a force of attraction of some kind, and to have been deflected from its straight path." But let him replace these glasses by others of Einsteinian make, and he will say: "No, I see now that there was no force of attraction at all. It was purely the inertia of the moving particle combined with the peculiar curvature of the surface which it had to traverse that produced the change in its path."

In the later development of Einstein's theory there is to be found a tendency to say not that a particle of matter has a space-cusp surrounding it, but that the cusp itself constitutes what we call a material particle. On this view the equivalence of matter and energy follows easily. Matter is static, an initial distortion in "space"; energy is kinetic, the spreading ripple into which the initial distortion is converted when whatever is holding it lets go. On this view there is little to choose between the old concept of an ether and Einstein's concept of space. If space can be bent it may be straightened, and if this process be repeated frequently enough the space may be said to vibrate. Endow Einstein's "space" with resiliency as well as deformability, and we have something which strangely resembles the old-fashioned "ether."

But what happens when energy is reconverted into matter, as we have seen must take place in the electrolysis of water, or in any process which involves an increase in the potential energy of the system? It is not inconceivable that if the amplitude of the energy waves reaches a certain intensity the medium which carries them, call it space, ether or what you will, may acquire a permanent or quasi-permanent distortion, like a body which has been strained beyond its elastic limit. Such a distortion may slowly straighten out again under the stimulus of passing waves, perhaps by discrete

jumps, as the quantum theory demands, much as a pile of cannon balls may be conceived to disintegrate under the influence of a mild and continuous earthquake, one ball at a time being dislodged and rolling down. In particular, such an intensity might conceivably be reached if our space has a slight positive curvature, analogous to that of a sphere; for then radiation starting from any point must eventually converge to the opposite "pole" of the universe, where its intensity must be as great as at the starting point. It is a curious idea, this of matter distilling, so to speak, from one pole and condensing at the other, through the intermediate phase of radiant energy. It possesses at least this recommendation, that it holds out a way of escape from the intellectually intolerable position of having to suppose that the ultimate fate of radiant energy is to travel, like the Wandering Jew, onward for ever.

"Upon this supposition of a positive curvature," said Clifford, fifty years ago,[5] "the whole of geometry is far more complete and interesting. . . . In fact, I do not mind confessing that I personally have often found relief from the dreary infinitudes of homaloidal space in the consoling hope that after all this other may be the true state of affairs."

[5] "The Postulates of the Science of Space"; Lectures and Essays.

OUR IMMIGRATION POLICY AND THE NATION'S MENTAL HEALTH

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SINCE the development of the first settlements in the North American continent efforts have been made to prevent the introduction of undesirable elements into the population. Attempts were made by the colonies, the several states, and finally our national government undertook to skim the dross from that ever-increasing mass of humanity which sought admission to the new world. A distinction is made, however, between those who took part in building the political framework of the thirteen colonies and the Federal Union and those who arrived to find the United States Government and its social and political institutions in working operation. The former have been called "colonists" and the latter "immigrants."

The menace of the dependent and defective classes who arrived in this country during the colonial period influenced the colonies to enact laws for the support of their poor and impotent and to impose penalties upon masters of vessels for bringing undesirable persons into their provinces. Irrespective of the liberal welcome accorded new settlers during that period, colonial laws all bear witness to a unity of opinion concerning the exclusion of dependent and defective classes. Even at that early date it had become imperative to adopt a policy of imposing penalties upon common carriers for bringing in undesirable settlers. This policy was later adopted by the legislatures of the several states and finally by the national government itself.

After 1838, however, the necessity for excluding undesirable immigrants became more pressing. It was then that a great turning point occurred in the influx of immigration to this country. In that year the *Great Western* sailed under steam from Bristol to New York, and two years later Samuel Cunard crossed in his first steamship, the *Britannia*, from Liverpool in fourteen days. The advent of transatlantic steamships lessened the terrors of an ocean voyage and reduced the cost of an emigrant's passage from pounds to shillings.

Until 1882 the majority of immigrants came from northern and western Europe. This movement of peoples has been called the "old immigration." For many years they were predominately

Irish, the English, Welsh and Scotch following closely in aggregate numbers. Immigration from the British Isles continued the dominant group until 1854, when their numbers were exceeded by emigration from Germany. The immigration occurring since 1882, however, has been associated with an increasing number of persons from southern and eastern Europe. This movement of peoples is sometimes termed the "new immigration."

The separateness of life and habits of newly arrived immigrants has usually invited antipathy from the natives, who tend to develop a feeling of contempt toward all immigrants of the poorer class, irrespective of their race. To the mind of the average native American, the typical immigrant has been and is regarded as a being uncleanly in habits, uncouth in speech, lax in morals, ignorant in mind and unskilled in labor. The immigrant has often borne a stamp of social inequality, suggesting an impersonal antipathy on the part of the native-born.

This sentiment, which existed toward the old immigration as well as toward the new, eventually crystallized the essential features of "nativism," which first gave rise to the so-called "Nativist" and "Know Nothing" movements. Through the efforts of those supporting the nativist movement memorials were sent to Congress urging a repeal or modification of the naturalization laws and the passage of laws to prevent the introduction of undesirables from foreign countries. This led to the appointment of a special congressional committee to consider these questions.

This committee ascertained that Great Britain was legalizing the deportation of its paupers; that many immigrants were admitted to almshouses within a very short time after landing, in some instances within a few hours; that persons convicted of crimes in Europe were promised amnesty upon emigrating to the United States; and that criminals condemned to life imprisonment were taken directly from the prisons of Germany and deported to America. As a result of these findings the committee presented a bill to Congress on February 19, 1838. It proposed a fine of $1,000 or imprisonment from one to three years for any master who took on board his vessel with the intention of transporting to the United States any alien passenger who was an idiot, a lunatic, afflicted with any incurable disease or convicted of any infamous crime. It further provided that the master of the vessel should forfeit $1,000 for any alien brought to the United States who had not the ability to maintain himself. This bill was never considered. It is noted, however, that the memorials to Congress largely responsible for the appointment of this committee urged the adoption of a system of consular inspection of immigrants before embarkation.

It was during the late 40's and early 50's, when transatlantic steamships were bringing immigrants in ever-increasing numbers, that the sentiment against foreigners was revived and the so-called "Know-Nothing" movement became most active. Its supporters advocated laws restricting the immigration of the dependent, defective and criminal classes, but by 1858 this movement had disappeared, to be replaced in the early 60's by sentiment advocating unrestricted immigration.

In 1864 Congress authorized the importation of "contract labor"; the appointment of a commissioner of immigration to arrange for the transportation and care of immigrants until they reached their final destination; and preparations were made for the appointment of special agents in European countries to promote and assist immigration. This sentiment subsided with increasing immigration, however, and in 1868 the Act of 1864 was repealed. With its repeal public sentiment became more and more crystallized in its demands for regulating immigration.

The regulation of foreign immigration, which had been entirely under the jurisdiction of the separate states, was fast growing beyond their control and frequent requests were made for national aid of some sort. The public's demand for this aid was partly justified by an investigation conducted by the Department of State in 1874, when it was definitely proved that foreign officials of many European nations were deporting to the United States convicts, paupers, idiots, insane and others incapable of self-support. Congress protested these acts, and restrictive legislation was proposed but not enacted. Federal aid eventually came, however, for in 1876, by a decision of the Supreme Court, all state laws relating to immigration were declared unconstitutional and the authority for its regulation declared vested in the national government alone.

It was not until 1882, six years after state regulations were declared unconstitutional, that the first federal immigration law was enacted. It provided for the exclusion of foreign convicts, lunatics, idiots and persons likely to become public charges. Several defects existed in the first federal immigration law, however. No penalties were imposed for the illegal landing of excludable aliens, and no provisions were made for the temporary care of immigrants. Local agents who conducted inspections and examinations were appointed by their respective states, but were neither paid by the states nor by the federal government.

These defects were not corrected until 1891, when a new law was enacted. It debarred idiots, insane persons, those insane within three years after admission, those having had two or more attacks of insanity, those suffering from loathsome or dangerous contagious

diseases, polygamists, felons and those who had been convicted of crimes involving moral turpitude. Penalties were imposed upon persons bringing aliens not lawfully entitled to enter, and medical examinations at ports of arrival were henceforth to be conducted by officers of the United States Public Health and Marine Hospital Service. A notable feature of this law was that transportation companies were required to return all persons coming unlawfully and, in addition, to return those who became public charges within one year after landing. It also prohibited common carriers from soliciting emigration except by ordinary advertisements.

This law was further elaborated in 1893 when masters of vessels carrying immigrants were required to give more detailed reports of each passenger. In 1894 the President was authorized to appoint immigration commissioners at the several ports for a term of four years, and in 1895 to appoint a commissioner general of immigration. It was also about this time that a proposed system of consular inspection of immigrants was revived. A bill proposing such a system was introduced in the second session of the fifty-third congress, but being opposed by the state and treasury departments, it failed to pass.

In 1896 a literacy test was recommended as a means of excluding that class of immigrant which investigation had shown contributed most heavily to pauperism, crime and juvenile delinquency. This feature in the regulation of foreign immigration was not enacted into law until some years later, however. In 1903 Congress saw fit to modify the immigration laws. During that year it increased the period of possible deportation of those insane within five years after landing and added professional beggars and anarchists to those already excludable.

No further changes were made in the immigration laws until 1907, when stricter measures were taken to prevent the importation of undesirables. Even this law was not adequate, for the problem of immigration was becoming more and more a problem of national importance. In 1911 the United States Immigration Commission was appointed. It made an intensive study of immigration and published a voluminous and illuminating report. In February, 1917, largely resulting from the work of this commission, the Act of 1907 was broadened in scope. It made more far-reaching provisions for the deportation of those having been sentenced to terms of imprisonment for crimes involving moral turpitude, and those becoming public charges within five years after landing. It excluded the insane; idiots; imbeciles; feebleminded; chronic alcoholics; constitutional psychopathic inferiors; the mentally defective whose defect would modify their ability to earn a living; those

with loathsome or dangerous contagious diseases, and those over sixteen years of age who were without a reading knowledge of some language. Thus, the literacy test, first proposed in 1896, eventually became a law.

From a review of the immigration laws considered thus far, it is evident that a unity of opinion has existed for skimming the dross from the peoples of Europe who seek admission to our country. All legislation seems to have been fostered by demands for the exclusion of the mentally and socially unfit. Regardless of the improvement in legislation, however, mentally disordered persons continue to seek admission. Earnest attempts have been made to exclude these undesirable aliens in two ways; by examinations at the port of arrival, and by imposing penalties upon common carriers for bringing them. Partial success has been achieved by these measures, but no machinery has been developed that will exclude those potential misfits or doubtful cases who possess latent qualities for injury to the community or national welfare.

A measure of the success attendant upon the examinations at ports of entry may be obtained by an analysis of the certificates issued for mental disorders by those concerned with examinations at the ports of arrival. During the 22-year period from 1900 to 1921, 17,957,807 arriving aliens were inspected and examined by officers of the United States Public Health Service. Of this number 6,629, or 36.9 per each 100,000, were found to be afflicted with mental disorders of one kind or another. These do not embrace the doubtful cases, but only those who are mandatorily excludable under the law.

The rate of debarment for some of the principal racial groups is illustrated in Fig.1, which indicates that the northern and western European generally contributes a higher proportion of mental disorders detected at ports of entry. The Mexican and Irish races suffer most from the hardships and inconveniences of deportations because of mental disease.

In addition to the restrictive measures embodied in the examination of aliens at ports of entry, the immigration laws have imposed certain penalties upon common carriers for transporting any of the excludable groups. A fine of $200 was imposed by the law of 1917 for transporting a mandatorily excludable alien whose disability could have been detected by means of a competent medical examination abroad. In addition, a sum equal to that paid by the alien for his passage from the initial point of departure was collected and returned to the immigrant. A penalty of $25 was also imposed for bringing any person with a mental or physical defect which might affect his ability to earn a living, provided that such defect

could have been detected by competent medical examination before embarkation. The master, or first officer of the vessel, was required to verify the ship's manifest or passenger list by oath and signature. This oath attested that the ship's surgeon had been required to make a physical and mental examination of each alien passenger and that no alien embarked who was excludable under the law.

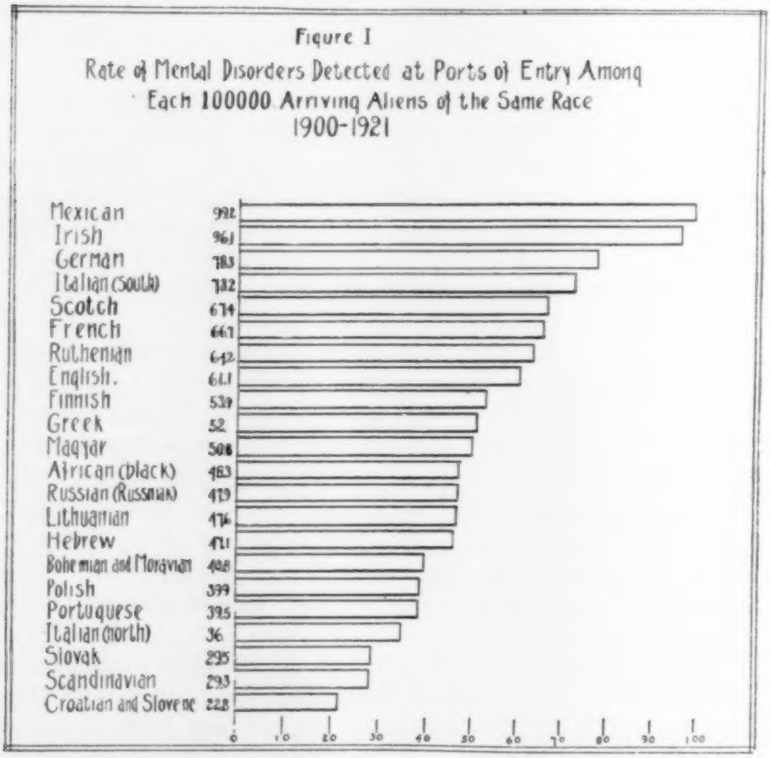

FIG. 1

The value of the examinations of aliens at ports of entry and the imposition of penalties upon common carriers for transporting mandatorily excludable aliens may have influenced the number of foreign-born admissions to public hospitals for the insane who had resided in this country for varying periods of time. For example, in New York state, among all foreign-born admissions to institutions for the insane in 1909, 22 per cent. had been in the country less than five years; 13 per cent. for five years but less than ten; 20 per cent. for ten years but less than twenty; and 43 per cent. for twenty or more years.

In 1920, however, this situation had changed. In that year only 4.7 per cent. of the foreign-born admissions had been residents of

this country for less than five years; 17 per cent. for five but less than ten years; 29 per cent. for ten but less than twenty years; and 35 per cent. for twenty or more years. But these changes are greatly influenced by differences in the length of residence of the foreign-born population for the two periods. Thus, in 1910, 25.3 per cent. of the foreign-born population of the United States had been residents for less than five years, and only 14.3 per cent. of the admissions to all institutions for the insane in the United States had been residents less than five years. In 1920, only 5.7 per cent. of the foreign-born population of New York state and 4.7 per cent. of the insane first admissions to New York state hospitals had been residents less than five years. Those within the second five-year period of residence, however, contribute a proportionately higher number of insane. Among those who had been residents of the United States for ten or more years, the number of insane recruited therefrom was proportionately greater in 1910 and proportionately lower in 1920. How great a factor the regulation of foreign immigration was in influencing this situation can not be accurately determined from data at hand.

Since the foreign-born population contributes a relatively high proportion of insane among first admissions to institutions, it is likely that among all arrivals an unknown proportion possess potentialities for the development of mental disease. Congress evidently recognized the necessity for excluding those who were potential misfits and also the doubtful cases who possessed latent qualities for injury to the community or national welfare. Thus, the new immigration law, which became effective on July 1, 1924, imposed greater penalties upon transportation companies for bringing excludable aliens to this country. The new law provided a penalty of $1,000 upon common carriers for bringing an alien with an excludable disease that could have been detected by competent medical examination at the time of embarkation. This penalty included a sum equal to that paid by the alien for his transportation, the latter sum being returned to the immigrant. The rigid enforcement of this feature of the new law will probably act as a deterrent to steamship companies accepting as passengers those in whom a reasonable doubt exists regarding their physical or mental health. The excludable groups comprise "any alien afflicted with idiocy, insanity, imbecility, feeblemindedness, epilepsy, constitutional psychopathic inferiority, chronic alcoholism, tuberculosis in any form or a loathsome or dangerous contagious disease.

But the new immigration law also provides for a system of consular visas of immigrants' passports and issues such visas in keeping with the quota of each nationality. The issuing of a consular indorsement does not permit an alien entry to the United States if he is otherwise an inadmissible person. These visas are of two kinds: those for "quota immigrants" and those for "non-quota immigrants," the act specifically defining both classes. A system of consular inspection abroad, first advocated in 1838, may prove to be of great value in excluding potential misfits from our shores. However, the withholding of a consular indorsement, particularly for those with frank mental and physical disorders, is certainly a humane procedure and will tend to lessen those hardships and disappointments incident to future deportation proceedings.

Any scheme for the better selection of immigrants, however, must consider that probationary period through which an alien passes before becoming a citizen of the United States. At present, if he becomes a public charge within five years after arrival he is subject to deportation. Other social tests besides that of becoming a public charge may be used to determine his desirability for citizenship. Such tests may be utilized eventually to determine whether an alien may or may not continue his residence in this country.

THE ATTACK ON THE GENE

By Professor JAMES W. MAVOR

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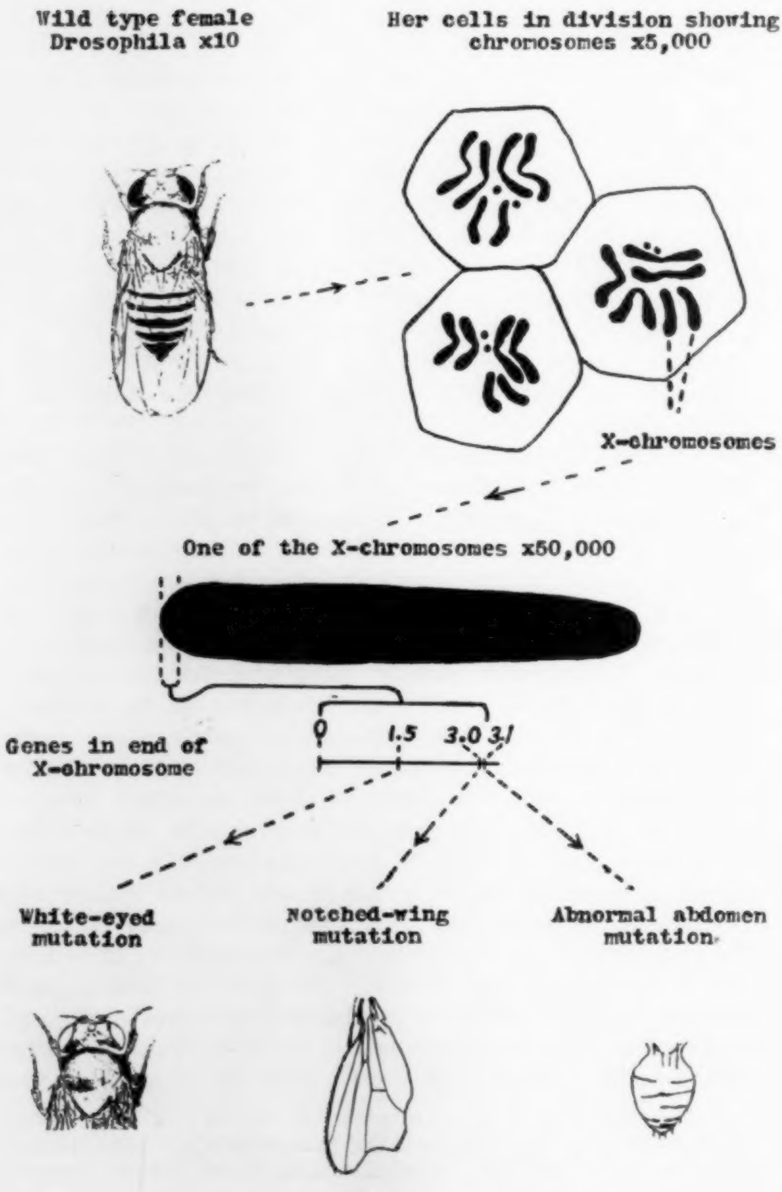
At the present time biologists are intensely interested in little things which they call genes. The conception and demonstration of the gene has crystallized many vague ideas and put the whole subject of heredity on a concrete basis. The genes have therefore in the minds of the students of this subject come to assume a most important rôle. They are the ultimate elements, the carriers of the unit characters. Upon the possibility of their alteration by experimental means rests the real answer to the question whether man can or may expect to modify heredity. And since the issue has become so clearly defined there is a feeling among those who are working in this field that at any moment very important discoveries may be made.

During the last twenty years the intensive study of inheritance has drawn more and more attention from professional biologists. Their combined efforts have now provided us with a fairly clear and detailed picture of the mechanism by which at least a considerable proportion of the hereditary characters are transmitted. Most is known about those characters which have been called unit characters, since they behave in a way as units, being passed from generation to generation without visible alteration and in such a way that an animal or plant either has or has not the character. Corresponding to these unit characters are certain minute bodies located in the germ cells, and indeed in every cell of the body, which are responsible for the transmission of the characters from one generation to the next. These are the genes. So far it has not been possible to demonstrate the genes microscopically, but they are known to be aggregated in certain clearly defined bodies called chromosomes. Chromosomes have been studied under the microscope not only in dead cells where minute details can be seen but also in living cells where their movements in cell division can be followed. It has further been possible to demonstrate by statistical methods that the genes are arranged in the chromosomes in a certain definite order.[1] So there is now no doubt in the minds of those

[1] Regarding figure 1, it is to be noted that, notched wing being dominant, the gene for it could not be present in the chromosomes of a female showing the characters of the wild type. White-eyed, being recessive, might however be carried by such a female. Abnormal abdomen although dominant depends for its appearance on certain environmental conditions. The outlines of the chromo-

Chromosomes and Genes in Drosophila



competent to judge that the hereditary characters are real things and that they are controlled by genes located in the chromosomes. This detailed knowledge of the mechanism of inheritance is by no means confined to the lower animals and plants. Many human characters are known, and the number is being constantly added to, which are transmitted according to the well-known laws of heredity. Microscopic study of the chromosomes of the human germ cells has shown that they agree in structure and mode of distribution with what has been generally demonstrated for animals.

There are few fields into which the methods of science have penetrated which have a more direct bearing on man's destiny than that which has to do with the handing on and possible modification of the great heritage which he carries in his own body and bequeaths to his children. The attempts to modify heredity have necessarily been made on the lower animals, but there is no doubt that the general conclusions to which they lead apply equally to man. It may be remarked incidentally that in none of the experiments discussed in this article have the animals been given any painful treatment.

We are all more or less familiar with the method of experimental breeding. When animals or plants with a number of unlike hereditary characters are bred together their offspring usually possess a mixture of the characters of their parents, but the individual characters as, for example, color and structural peculiarities, when expressed retain their individuality and are passed unaltered through successive generations. Such a method does not in itself introduce anything essentially new. It produces only a different combination of characters which were already present in the parents. A second method which has been tried is to produce some modification in one or both parents in the hope that this same modification may be detected in the offspring. In some cases the parent is injured, as in Weismann's famous experiment with mice from which the tails were cut. In other cases the modification takes the form of an apparent adaptation to a new environment, as in Dr. Kammerer's experiments with salamanders. With regard to experiments of this kind it is safe to say that those who have gone into the matter agree that there is no well-substantiated case of an experimentally produced modification of a parent being transmitted to its offspring.

In all the cases in which it is admitted that a modification of heredity has been produced it is the germ cells which have been

somes are from Bridges, *Genetics*, Vol. 1, the wild fly from Morgan, "The Physical Basis of Heredity," the notched wing from Dexter, *American Naturalist*, Vol. 48, and the abnormal abdomen from Morgan and Bridges, Carnegie Institute publication, no. 237.

themselves directly modified. The external agent or some factor in the environment has acted directly on the germ. It is true that in some cases the body of the parent may also have been affected, but the modifications of the germ cells and the parent body are collateral, so that there is no question of an adult animal being modified in such a way as to transmit the modification to its offspring. The distinction made here may seem to be somewhat academic. It is none the less fundamental and essential to a clear understanding of the problem. A good case in point is that provided by Dr. Stockard's experiments. Guinea pigs were intoxicated with the vapor of alcohol on successive days over a considerable period of time. The offspring of animals treated in this way showed various abnormalities, chiefly of the nervous system and eyes. The animals with these abnormalities did not breed. However, apparently normal offspring from the treated animals had abnormal young. Dr. Stockard's explanation is that the reproductive organs and germ cells became injured. It is known that alcohol can penetrate through the tissues of the body and reach the germ cells. Dr. Guyer's experiments on rabbits, although very different from those of Dr. Stockard, are nevertheless probably to be explained also as a case of a direct effect on the germ cells. Pregnant rabbits were injected with a serum made antagonistic to the development of the lens of the eye. Certain of the young from such rabbits showed defects which they transmitted to their offspring.

In experiments of the kind described it has not been possible, up to the present, to analyze the results in such a way as to determine what part of the hereditary mechanism has been affected or to determine whether one or more genes have really been modified. It is quite possible that in these experiments there may have occurred only a loss or an abnormal distribution of chromosomes as in experiments to be considered later in this article. It may, however, be remarked that Dr. Little has studied the inheritance of certain defects which occurred in the offspring of mice exposed to X-rays, but he does not claim to have proved that the abnormalities were due in the first instance to X-rays or any external agent. This method, then, of attacking the problem, *viz.*, by treating normal parents or their germ cells with modifying agents such as alcohol, serum and X-rays, has so far not yielded any information as to the way in which the external agent modified the germ cell.

Mention has already been made of the mechanism of heredity—of how each unit character, as, for example, a particular color of the eye or body, is carried over from parent to offspring in a gene. The genes have been located in the chromosomes and each germ cell, be it an egg cell or a sperm cell, carries a full complement of the

chromosomes characteristic of the species. In the case of the common fruit fly (*Drosophila melanogaster*) somewhat over three hundred different unit characters have been investigated and the genes corresponding to them located in one or other of the four different chromosomes found in the mature eggs and sperm. The sperm cell when it fertilizes the egg brings with it its contribution of four similar chromosomes so that the fertilized egg and all the millions of body cells into which it divides to form a new individual each contains eight chromosomes, forming, however, four pairs, one of each pair having come from the mother and one from the father. By this microscopic mechanism the offspring inherits in a general way equally from its two parents. This statement must be qualified. While the genes occur in pairs, one coming from the mother and the other from the father, they may not be exactly the same so that while the maternal chromosome may carry in it a gene which causes the offspring to have red eyes the corresponding chromosome derived from the father may carry in place of a gene causing the formation of red eyes one which would cause the eyes of the offspring to be white. In such a case the eyes of the offspring are red in spite of the fact that only one of the pair of chromosomes carries the gene for red eyes. In this case the character and the gene are said to be dominant. If however such a hybrid individual is bred the white eye or recessive character will come out in its offspring. An animal or plant will continue to breed true only when the genes in both its chromosomes are alike.

There is a further complication involved in the transmission of certain of the hereditary characters and in the determination of sex. We referred to the fact that each mature germ cell of the fruit fly contains four chromosomes and each fertilized egg cell and each of the cells of the individual to which it gives rise contains four pairs of chromosomes. When now the germ cells are matured in the body of this individual each has again only four chromosomes, the members of each pair of chromosomes having been separated in the process of maturation. In the cells of the body of the female the two chromosomes of each pair are exactly alike, although they may not contain identical genes. In the cells of the body of the male on the other hand the two chromosomes of one of the pairs are not alike, one of them being identical with each of those in the corresponding pair in the female. This chromosome has been named the X chromosome and it is usual to speak of the cells of the female as containing two X chromosomes and those of the male as containing one X chromosome and one Y chromosome. It therefore comes about that when egg cells are matured in the female they are all alike and each contains one X chromosome, while when

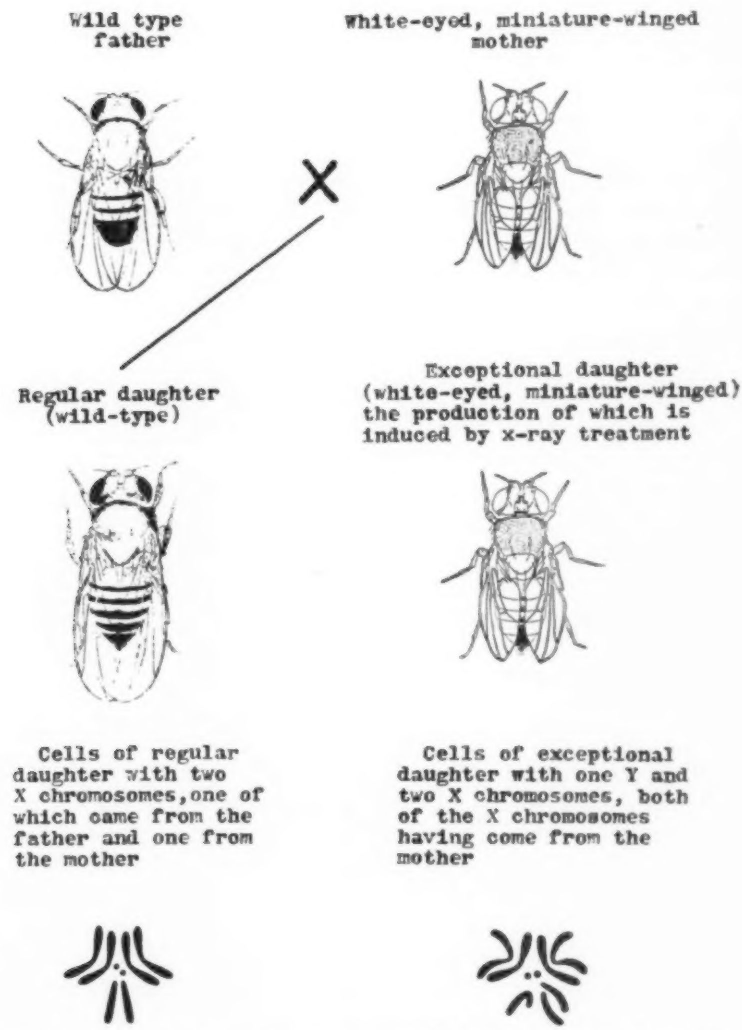
sperm cells are formed in the male they are of two kinds, one kind containing an X chromosome and the other kind containing a Y chromosome and that when an egg is fertilized by a sperm cell containing an X chromosome it comes to have two X chromosomes and to be a female, while an egg which is fertilized by a sperm cell containing a Y chromosome comes to have an X and a Y chromosome and develops into a male. It appears further that no genes are carried in the Y chromosome. Such is the mechanism by which unit characters are transmitted from parent to offspring in the fruit fly. While the number of pairs of chromosomes differs in different species the mechanism is believed to be fundamentally the same throughout the animal and vegetable kingdoms.

With every germ cell nature deals to the future individual a hand of cards. These cards are the genes, and as his body plays the game of life the cards come out and some, standing high, are dominant, as we say, and take the tricks, and some, standing low, are recessive, and show their value only in the presence of other low cards. For each new deal the cards are shuffled, but they are in almost every case the same old cards.

Thus is insured not only that the parents in general equally contribute to the heritage of their offspring but also that this heritage becomes equally distributed among all the cells of the offspring and all their germ cells. This aspect of the biology of the cell has been worked out with the most minute detail of recent years and its application to explain the immense accumulated knowledge of the external phenomena of heredity constitutes one of the great accomplishments of biology during this century. So extensive is now the data and so confident are those who work with it that it is possible to say that a given hereditary behavior, *i.e.*, certain results for certain crosses, implies the corresponding microscopic processes in the germ cells. To be more precise if a given male fruit fly has white eyes it is certain that in its cells there is but one X chromosome and that in this X chromosome lies the gene for white eye, and in a female fly with white eyes it is possible to be certain that in the cells of its body there are two X chromosomes and that each of these X chromosomes has the gene for white-eye color.

Now this little mechanism with its genes and chromosomes and the forces which keep them apart or bring them together is just the sort of thing with which the chemist and physicist delight to experiment. Processes which depend, like those which govern the distribution and segregation of the genes, on delicate adjustments of chemical and physical forces, yield readily to modification and control by modern technical methods. The difficulty of the problem lies not in finding methods calculated to modify and control

The Effect of X-Ray Treatment of the Eggs upon the Offspring of the Fruit Fly, Drosophila



the mechanism of inheritance, but to apply methods in such a way as not to destroy the life of the cell.

In the experiments to be described next a beginning has been made in this fascinating and romantic field. The physical agent applied to the cells was X-rays, long known to produce striking effects on the chromosomes. As is well known, X-rays have the peculiar property of penetrating to all parts of the animal. It is

therefore only necessary to expose the entire animal to the X-rays in order to treat the germ cells, and indeed the germ cells are so sensitive to X-rays that an exposure which causes marked changes and even death in the germ cells leaves the rest of the animal practically unaffected. Such experiments require, however, not only a critical application of physical forces but also the careful choice of an animal in which slight changes in the hereditary mechanism can be observed. Such an animal is the common fruit fly already referred to.

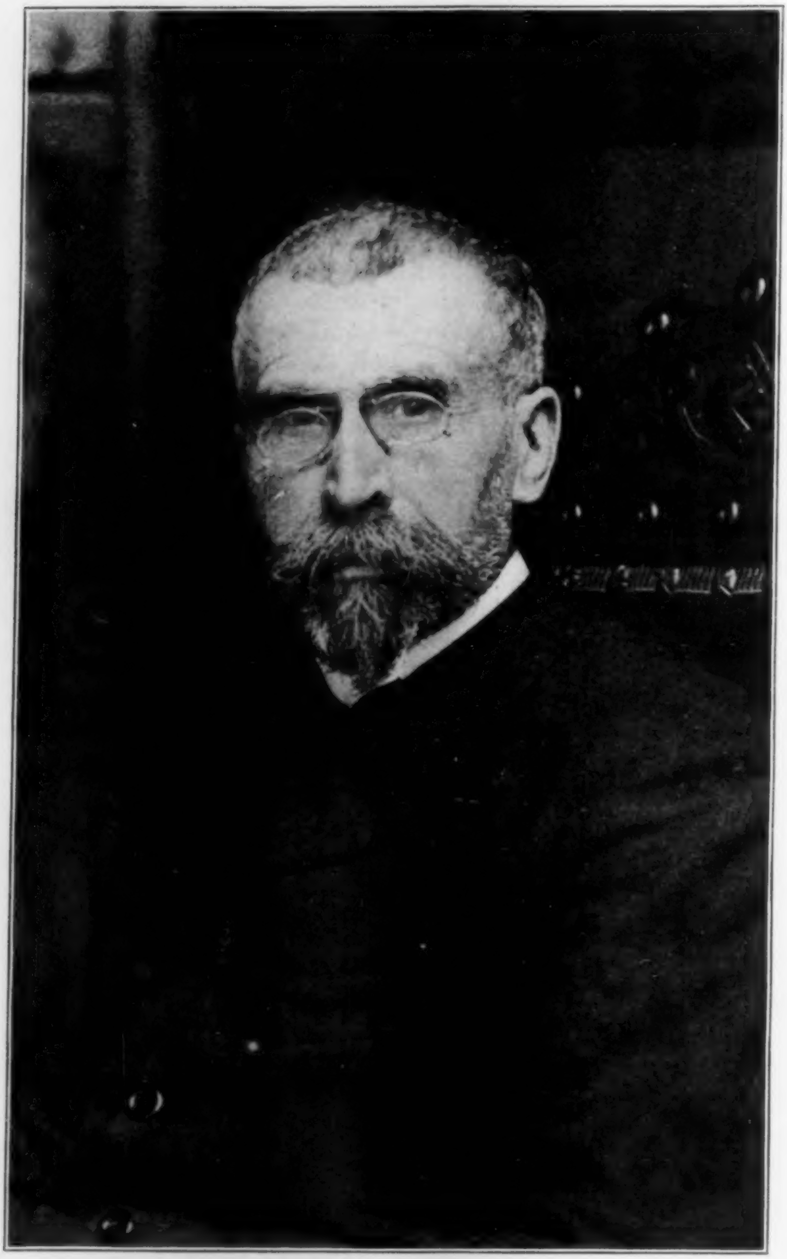
Considerations such as those outlined above led the writer to treat with X-rays fruit flies whose hereditary constitution, so far as certain well-known unit characters were concerned, was exactly known. The unit characters chosen were white-eyes as contrasted with normal red-eyes, and miniature wings as contrasted with normal wings of which they are about half the size. The genes for these characters are located in the X-chromosomes. Female flies were treated and in this way their eggs were exposed to X-rays before fertilization. When these were bred to normal males the exact effect on the offspring could be observed, from which it was further possible to determine the effect on the chromosomes and arrangement of the genes among the chromosomes. The experiments have shown that one effect of the X-rays is to cause the elimination of the X chromosome from the egg or to cause the formation of mature eggs with two instead of the usual one X chromosome. Such eggs give rise to flies peculiar in their hereditary characters and the way in which they breed but otherwise normal. Another effect of the X-rays is to alter the assortment of the unit characters among the different offspring—an effect that can be traced to a decrease in the frequency with which the chromosomes may break into pieces. However, in spite of these peculiar and rather profound effects on the mechanism of the inheritance of unit characters no conclusive evidence has been obtained that any individual gene or unit character has been altered, although during the course of the experiments somewhat over one hundred thousand flies developed from X-rayed eggs have been studied.

The conclusion to be drawn from all these investigations is that in no case has it been demonstrated that an individual gene has been modified by experimental means. It can equally be said, however, that there is no proof that the gene can not be so modified, and we know that genes do naturally change or as we say mutate.

Seldom do those who follow science turn back from the attack upon the problems to which the advance of knowledge leads. So the assault upon the gene will continue. What form will it take? At the present moment there are two promising lines of work. The

first is chemical and consists in searching for some substance which while penetrating through the cells of the body and into the germ cells reacts specifically with certain of the genes. The other method is physical and consists in attempting to find a physical condition such as a particular pressure or temperature which affects only certain genes or a kind of ray or other mode of energy which can be concentrated in the cell in a point of sufficient fineness to affect a single gene or a small group of genes. If any kind of radiation were used the point of the beam would have to be less than one millionth of an inch in diameter. It is essential that only a single or at most a few genes be affected, since we know that sorted in with the genes of the hereditary characters which we recognize as such are other genes, probably more numerous, whose disturbance leads to death. What a delicate adjustment is life and how refined must be any attempt to interfere with its fundamental processes!

There is a feeling prevalent not only among laymen but also among members of the medical profession that experiments on "the lower animals" throw little light on human problems. This feeling is undoubtedly changing and may soon be a thing of the past. Even now the up-to-date medical man looks at his subject from a "biological point of view" and our schools of social science give courses in "civic biology." One has only to pass over in his mind the discoveries which have most advanced our knowledge of "the nature of man" to realize how large is the proportion due to biologists in the larger sense of that word as used now. Nor has the immediate and direct application of their discoveries been less striking. Whether we will or not biology is moulding our lives.



DR. ÉMILE ROUX
Director of the Pasteur Institute in Paris for twenty years (1904–1925).

THE MEDICAL WORK OF PASTEUR[1]

By Dr. ÉMILE ROUX

DIRECTOR OF THE PASTEUR INSTITUTE, PARIS

(Translated from the French by Erwin F. Smith, U. S. Department of Agriculture, Washington, D. C.)

THE medical work of Pasteur begins with the study of fermentations. From the beginning men have associated fermentations with infectious diseases and regarded viruses and ferments as of the same nature. But between fermentation and disease there is this difference, that the fermentable medium is inert, that we can compound it as we please with definitely weighed-out materials while the body of the infected animal is living and of a complexity hitherto impossible to penetrate. Consequently, the study of fermentation is easier than that of infectious disease and naturally precedes it.

[1] A generation has passed since this paper was written (1896) but it is still as interesting as ever. It is, in fact, the best brief paper we have on the work of Pasteur and the writer of this note believes many will like to have it in an English dress.—E. F. S.

The various doctrines on the origin of life have always run up against the doctrine of fermentations. No other question has more excited religious and philosophic passions than that of the ferments, which, we must acknowledge, do not diminish their obscurity. Pasteur understood that on such a subject he ought not to advance anything which did not rest upon clear proofs and from the beginning he restricted himself to a scientific method which resolved each difficulty by an experiment, simple to interpret, an experiment which charmed the mind and at the same time was so decisive that it satisfied him like a geometric demonstration and gave him a feeling of security.

It was, therefore, from 1857 to 1865 that the great struggles took place over fermentations for the principles out of which has come forth triumphant the doctrine of germs. Pasteur had already revolutionized medicine before having undertaken the study of a single disease. Each one of the fundamental principles established for fermentations applies with the same exactitude to infectious diseases; in fact:

The virus is a living being like the ferment. Both are microbes, as we would say to-day.

The virus by multiplying in the body causes the infectious disease, as the ferment multiplying in a fermentable medium causes the fermentation.

To each infectious disease there corresponds a specific virus, as to each fermentation a special ferment.

The virulent disease is not spontaneous, any more than the fermentation. The virus comes from outside, and consequently contagion can be avoided.

The extension to diseases of notions acquired in the study of fermentations brought about unheard-of progress. Surgery, whose interventions so often provoked redoubtable infections, became henceforth beneficent. Enlightened by the experiments of Pasteur, Lister comprehends that the complications of wounds are due to microbial germs coming from outside and he invents antiseptic dressings. With antisepsis begins the new time in surgery. Thanks to an experiment on fermentations, well done, Pasteur, who had never touched a scalpel in his life, nor made a surgical dressing, has saved more wounded than all the masters of surgery.

These labors of Pasteur upon the fermentations have given to medicine not only a doctrine, but also a method of research and a technique of marvelous power.

In order to study fermentation, Pasteur prepared first of all a *pure* fermentable substance, that is to say, one which did not contain any living thing. He obtained it in these conditions by steril-

izing it with heat. Thus heated, this nutrient medium preserves itself unchanged, indefinitely. Pasteur then sowed it with a trace of the ferment *in a state of purity,* that is to say, containing no other microscopic organism, which, developing along with it, might falsify the result of the experiment. This necessity for *pure cultures* imposed itself on Pasteur from the beginning of his experiments and led to the most ingenious inventions. To succeed in obtaining a pure culture how many difficulties had to be overcome! How could any one prepare sterile organic infusions if he did not know why they ended by changing under ordinary conditions? It is in the celebrated memoirs on organized corpuscles in the atmosphere and upon spontaneous generations that Pasteur has unraveled the causes of the alteration in organic infusions and laid the foundations of the fertile method of bacterial cultures.

We preserve piously in the Pasteur Institute some flasks containing fermentable liquids prepared by Pasteur himself in 1860 for the Commission of the Academy of Sciences before whom he had called his opponents. We may read still upon their yellowed labels the signature of the illustrious chemist Balard, reporter for the commission. After thirty-five years, these liquids are clear as on the first day and are witness of the certainty of the methods of Pasteur.

The culture of microbe-ferments led to the culture of microbe-viruses. The latter, it was thought, were formed only in living matter, proceeding from it by way of a chemical change. The new doctrine, on the contrary, regarded the viruses as parasites, developing in the body of the diseased animals, and capable without doubt of an independent life. The procedures which had succeeded with the ferments permitted also the culture of the viruses outside of the organism, in artificial media, where they multiply or swarm in successive generations, without ceasing to be deadly for man and animals. The culture of viruses in glass tubes, in a definite nutrient substratum, at the will of the experimenter, who becomes, so to speak, a gardener of microscopic pathogenic plants, constitutes the wonderful means of investigation which is to renew everything.

Thus prepared by the study of ferments, armed with a technique of incomparable surety, it would seem that Pasteur had only to attack contagious diseases. His thoughts, as a matter of fact, return without ceasing to this subject; but he was not a physician and he dared not advance upon a territory reserved to pathology. It was in spite of himself that he entered it in 1865. It required all the influence that Dumas had over him to decide him to do it; after much resistance, Pasteur, who had never dissected an invertebrate, never even touched a silkworm, set out accompanied by his faithful

assistants of the Normal School, Duclaux, Gernez, Raulin and Maillot, to study the disease which was destroying the silkworm industry of southern France and which extended into all the silk-growing countries. His hesitations once overcome, Pasteur set about the work with an ardor which finally ended in compromising his health.

Cornalia and other scientific men after him had found, long before, in diseased worms little oval corpuscles visible under the microscope. But from this observation nobody had drawn any conclusions by way of avoiding the disease or of curing it. Pasteur devotes himself to these corpuscles: they are for him the parasite, cause of the disease. He sees them in the diseased worm, follows them into the chrysalis, and into the winged insect and also into the eggs laid by the latter. It is therefore the direct transmission of the corpuscles from the perfect form of the insect to the egg which renders the disease hereditary. To obtain healthy eggs it sufficed to separate the layings of each female and to preserve only the eggs from the perfect insect exempt from corpuscles. The exact observation of the facts led necessarily to procedures which saved the industry of silk-growing.

But those worms hatched from healthy eggs may become diseased if they are raised in infected localities, for the disease is contagious. It is contagious because the corpuscles pass into the excretions of the sick worms and penetrate with the foods into the digestive tubes of the healthy worms or are introduced into their bodies by scratches on the skin. Pasteur, in many experiments, obtained the contagion by means of food artificially contaminated.

How many teachings there are for human medicine in this study on the disease of the silkworm! This disease was no less mysterious in its cause and in its extension than the infectious diseases of man; like several of the latter it is hereditary and to explain it observers had not failed to invoke the epidemic genius, idiosyncrasies of the subject, etc. Without knowing anything of all these doctrines, a chemist, who knows how to look through a microscope and make experiments, shows that all this is reduced to a parasite transmitted by the diseased to healthy subjects and by parents to their descendants. The mystery of the contagion and of the heredity is thus explained.

The disease of *pébrine* is not the only one which ravages silkworms: it is often confounded with another disease, *flacherie*. Pasteur discovers that this latter is due to fermentation of the leaves of the mulberry in the digestive tube of the worm. The agents of this fermentation are microbes, a vibrio and an organism of a rounded form united into chains. The excretions of the diseased worms transmit the malady, which has the greatest analogy with

certain contagious diseases of man that also have their seat in the intestine. This disease is not hereditary, but it persists in the silkworm nurseries from year to year, although the eggs from which the worms are hatched may be exempt from the germ of the disease. This means that the vibrios of flacherie remain alive in the dry dust. These vibrios produce in their interior shining corpuscles like those already seen by Pasteur, in 1860, in the butyric vibrio. These corpuscles are the eggs of the vibrio; of an incredible resistance to heat and drouth, they perpetuate the species; to-day we call them *spores*. It is the first example of a spore in a pathogenic microbe and the rôle of this very resistant production in the transmission of diseases that appear only after a long time has not escaped Pasteur.

The book on the diseases of silkworms is a veritable guide to whoever would study contagious diseases. Nevertheless how few physicians had read it in 1876! Pasteur did not fail to say to those who entered his laboratory and whom he took for collaborators: "Read the Studies on the silkworm; it will be, I believe, a good preparation for the labors you are to undertake."

The disasters of the war of 1870 were very grievous to Pasteur and it was with a patriotic thought that he busied himself with the beer industry, in which heretofore Germans had been without a rival. His "Studies upon Beer" made of brewing a scientific industry; but their reach exceeds by far what the title would lead one to suppose. The chapters on the origin of ferments, on life without air and fermentation, the diseases of beer, are filled with new general ideas, as suggestive for the biologist and the physician as they are useful to the brewer.[2] Nevertheless the doctrine of Pasteur penetrated little by little into medicine; the success of Lister and of his pupils carried conviction and, more and more, audacious persons described microbes in infectious diseases.

With what attention Pasteur followed these first labors! They rejoiced him and they vexed him at the same time: These experiments of physicians appeared to him often defective, the methods seemed to him insufficient and the proofs not rigorous, suitable rather to compromise the good cause than to serve it. Soon he could not endure it and resolutely he set himself also to the study of anthrax.

Rayer and Davaine had described in 1850 in the blood of sheep which died of anthrax little transparent motionless rods. After reading the memoir of Pasteur on butyric fermentation Davaine, in 1863, recognized that these little rods are parasitic microbes which constitute the inoculable virus of the disease; he gave to them

[2] Pasteur was aided in his studies on beer by Gayon, Grenet and Calmette.

the name of *bactéridie charbonneuse.* Then Dr. Koch (since become so celebrated) saw the bactéridie multiplying outside of the organism in drops of the aqueous humor obtained from the eyes of rabbits; he succeeded in obtaining as many as eight successive cultures. The bactéridie of this eighth generation, inoculated into animals, gave them fatal anthrax.

We do not very well understand to-day how after these experiments one could deny that the bactéridie were the cause of anthrax. But in 1876 the minds of men were not prepared for the idea that the viruses are parasitic microbes. For the greater number of medical men the bactéridie were only an accessory of the disease. "But in the blood of animals attacked by anthrax," they said, "the rods of Davaine do not exist alone; by the side of them there are globules and the blood plasma which contains the true amorphous virus; the cultures of Dr. Koch do not suffice any more to convince us. In the drop of aqueous humor which he sows with the blood from the animal, diseased by anthrax, M. Koch brings at the same time as the bactéridie the virus contained in the plasma; and the successive sowings which he makes in drops of the liquid simply lead to a dilution of this virus. Now do we not know that it is the property of viruses to act in infinitely small doses and that they may be diluted prodigiously without extinction of their activity?" At the time when they were made these objections appeared full of force.

Such was the state of the question at the time Pasteur attacked the problem. He also made cultures of the bactéridie; but instead of sowing the anthrax blood in a drop of nutrient media he introduced it into a flask which contained hundreds of cubic centimeters of an organic infusion where the bactéridie multiplied in some hours. With a trace of this first culture he sowed a second culture and in like manner up to the twentieth and even to the hundredth generation. A little drop of this hundredth culture gives anthrax as certainly as does the blood of an anthrax diseased sheep. Here one may not invoke dilution of the virus; the primitive droplet has been drowned in oceans; what does there remain of it in this hundredth culture, mortal, nevertheless, in the most minute doses? The virus, therefore, reproduces itself. It is, therefore, a living being; it can be only this bactéridie which exists alone in the culture flasks. It is indeed that which kills and not chemical substances which accompany it. Let us place, in fact, one of these flasks which contain it in a place at a rather low and constant temperature, in the cellars of the observatory as Pasteur has done; all the bacteria in suspension soon fall to the bottom of the flask. The clear supernatant liquid, injected into animals, even in large quantities does not make them sick, while a little of the bacterial deposit introduced into

their bodies destroys them with anthrax. These simple demonstrations were possible only on account of a perfect technique. Pasteur and his collaborator Joubert made use of that which had so well succeeded in the fermentations. It was no more difficult for them to prepare a hectoliter of sterile nutrient media than to obtain some cubic centimeters. Their procedures are so sure that in a long series of cultures the bactéridie remain pure without any error being able to cause the result to be suspicious.

If the bactéridium is the cause of anthrax, its properties should enable us to understand the etiology of the disease. Davaine had very well understood that the new notions would not be definitely victorious unless they explained how the animals took the anthrax in the fields, how they there came across the bactéridium, how it later penetrated into them and finally why certain fields were ravaged, while others, quite near, were spared by the disease. The preponderant rôle that Davaine attributed in the propagation of the disease to flies, which settle on the bodies of the dead animals, left without explanation the most characteristic circumstances of the epidemics. His adversaries very soon caused him to see clearly that: "this theory does not explain all, therefore it does not explain anything." Davaine did not have a certain bit of knowledge without which the etiology of anthrax remains incomprehensible; that of the anthrax spore. The spore of anthrax was discovered by Koch, who saw it form in the bacterial filaments cultivated in contact with the air. This spore resists drying and heat, and the greater number of agents which destroy the bactéridie when in the form of rods. It is the resistant form of the microbe and it perpetuates the species.

For Pasteur, it perpetuates also the disease in the anthrax country. Had he not already seen germ-corpuscles of the vibrio of flacherie preserve themselves in the dust of silkworm nurseries and carry over the disease from one year to another? First of all, by experiments in the laboratory he showed that sheep which ate the spores along with their food contracted anthrax and showed the symptoms and lesions of the natural disease.

This anthrax spore should exist in the anthrax territories, it must be discovered there. It was not an easy enterprise, that of isolating some spores of anthrax from among the millions of microbes contained in each parcel of cultivated earth; it was successful, however, by putting to use the properties which the anthrax spores possess of enduring a temperature of 80° to 90°, which temperature kills the greater part of the microbes of the soil. Some of the soil of Beauce, classic land of anthrax, is put into suspension in water; the finest particles are gathered, heated to 80° and inocu-

lated into guinea pigs: some of which die of anthrax. It is, therefore, certain that the sheep in Beauce find the germs of anthrax in the pasture fields. But where do these germs come from? They come from the dead bodies of animals that the shepherds, following custom, bury in the open field where the beast has fallen dead. The filamentous bactéridie, innumerable in the blood, are carried with it into the aerated earth of the graves and thanks to the summer heat there rapidly form their spores.

From all these facts there results a very simple prophylaxis for anthrax. Do not bury the dead bodies of anthrax animals in the fields, but destroy them or bury them in restricted places. The soil of the pasture being no more provided with spores will cease to be dangerous.

This is what Pasteur never ceased to say to the farmers of Beauce, with whom he went to observe and to make experiments, for this etiology of anthrax has been established in the heart of the anthrax territory in the midst of shepherds and flocks. All this did not prevent contradictors from accusing Pasteur of being only a laboratory man, of seeing things over his retorts and his bouillons and not as they are in nature and in practice.

During several successive years, at the end of July, the laboratory of Ulm Street was abandoned for Chartres. Chamberland and I went there to live in company with a young veterinarian, Vinsot. We found there as guide M. Boutet, who knew his anthrax country better than anybody else, and we met there sometimes M. Toussaint, who studied the same subject we did. Every week Pasteur came to give directions and to follow the work. What pleasant remembrances these campaigns against anthrax in the land of Chartrain have left us! Beginning in the early morning, visits to the sheep pastures scattered over this vast plain of Beauce resplendent in the sun of August, autopsies performed in the knacker's yard in Sours, at M. Rabourdin's place or in the courtyard of the farms; after noon, drawing up of the experimental notes, letters to Pasteur, then the commencement of new experiments. The day was well filled and how interesting and salutary was this bacteriology in the open air!

The breakfast at the Hotel de France did not last very long the days when Pasteur came to Chartres; we were soon in the coach going to M. Maunoury's in Saint Germain, who had courteously put his farm and his flock at our disposal. During the journey we spoke of the experiments of the week and of those to be undertaken. Pasteur, as soon as he had set foot on the ground, full of haste, went to the sheepfolds; standing motionless close to the enclosures he gazed at the experimental groups with that sustained attention

which nothing escaped; for hours he followed with his eyes the sheep he thought diseased; we had to remind him of the hour and to show him that the spires of the cathedral of Chartres began to disappear in the night before he was willing to go. He questioned the farmer and the laboring men and always took into account the opinion of the shepherds who, on account of their solitary life, gave all their attention to their flock and often became very sagacious observers.

No fact appeared insignificant to Pasteur; from things slightest in appearance he knew how to draw unexpected indications. In course of a walk in a field on the farm at St. Germain was thus born the original idea of the rôle of the earthworms in the propagation of anthrax. The harvest was over, there remained only the stubble. The attention of Pasteur was drawn to a portion of a field on account of the different color of the earth; M. Maunoury explained that the preceding year they had buried in that place some sheep dead of anthrax. Pasteur, who always examined things very closely, noticed on the surface of the soil a multitude of little mounds of earth cast up by earthworms. The idea then occurred to him that in their continual journeys from the depths to the surface the worms would bring to the surface earth rich in humus which surrounds the dead body and with it the anthrax spores that it contains. This explains why the germs of the disease persist such a long time in the fields, although so many causes tend to make them disappear: they are brought to the surface of the graves by the incessant bringing up of deep earth. Pasteur never stopped with conceptions; he passed immediately to experiments. The latter justified his previsions. I remember, among others, demonstrations made before Villemin, Davaine and Bouley. The latter had brought worms that had fed on the earth of a grave where the bodies of animals dead of anthrax were buried several years before. The earth taken from the intestine of one of the worms when inoculated into guinea pigs gave them anthrax.

These researches on the etiology of anthrax will remain a model. Never hitherto had medicine known a like perfection in the experiments, a like rigor in the deductions and such a certainty in the applications. Pasteur knew very well that the decisive battle was on and he neglected nothing to make the victory certain. Not content with supporting all that he advanced with irresistible proofs, he desired to leave nothing obscure in the labors of others; he takes one by one the facts which seemed opposed to his doctrine and shows, sanely interpreted, that they confirm it and this work of control gives him the occasion for new discoveries.

It is thus that he goes over the experiments by which MM. Jaillard and Leplat had for a moment menaced the conclusions of

Davaine. Anxious to study anthrax, these experimenters had requested virus from Chartres. Blood of a cow diseased by anthrax was sent to them; they inoculated it into rabbits who died without showing bactéridie in the blood, and nevertheless this blood inoculated into other rabbits destroyed them. They conclude that the bactéridium is not the true virus of anthrax, since the disease may exist without it. Davaine examined these facts, recognized that the animals of Jaillard and Leplat had not succumbed to anthrax but to another disease. The conclusion of Davaine is perfectly correct; it does not dissipate all the obscurities, for it does not explain how anthrax blood inoculated has been able to cause a disease which is not anthrax. Pasteur, aided by MM. Joubert and Chamberland, is going to show us what takes place in the experiments of Jaillard and Leplat and at the same time disclose to us unexpected facts of the highest importance. When an animal, sheep or cow, is attacked by anthrax, its blood collected at the moment of death swarms with the bactéridie and gives with certainty anthrax to the animals into which it is inoculated. But already after some hours, especially in the heat of summer, putrefaction takes place, the microbes of the intestine invade the deep veins and then the other vessels. At this moment, the blood contains the bactéridie of anthrax no longer in a state of purity; with them is associated another microbe, a motile vibrio, which is always the first to pass from the intestine into the vessels. This vibrio is very deadly to rabbits; it kills them more quickly than the bactéridie. If, therefore, we inoculate a rabbit with blood taken from an anthrax cadaver some hours after death, the rabbit receives at the same time bactéridie and vibrios. It dies either of a mixed disease caused by the simultaneous development of the two microbes, or, more often, of a pure septicemia because the vibrio, multiplying more quickly than the bactéridium, eliminates it by a living competition. The septic vibrio swarms especially in the peritoneum and rarely invades the circulation until after death. These peculiarities explain to us how Jaillard and Leplat, having asked for anthrax blood from Chartres, have received septic blood and why in the blood of their animals they have not seen under the microscope the rods of Davaine any more than the septic vibrio which is found there only in small numbers.

This septic vibrio is an anaerobic organism like the butyric vibrio studied by Pasteur in 1861. It is killed by oxygen when it is in a filamentous state, but it produces spores which resist the air. It constitutes the first example of an anaerobic pathogenic microbe, and the procedures which have served for its cultivation will be later put to use in the study of other microbes living without air, such as those of tetanus and symptomatic anthrax.

This justly celebrated note by Pasteur, Joubert and Chamberland on septicemia and anthrax introduced into science the notion, since become very important, of microbial associations and also the idea of bacteriotherapy. We read in it, in effect, that when the anthrax bactéridie are inoculated along with certain other bacteria into animals, the latter may not take anthrax and that this observation will be perhaps the point of departure for therapeutic applications.

The experiments of Pasteur establish not only facts, they suggest especially ideas. Witness those of the chicken attacked by anthrax which caused such a great quarrel with M. Collin. Fowls are absolutely refractory to anthrax. Nevertheless, Pasteur, Joubert and Chamberland rendered them susceptible by lowering their temperature, which is 42° C., to 38° C. by immersion in water. It is the condition necessary and sufficient to enable the inoculated bactéridie to multiply in the body of the fowl. On the contrary, the fowl, chilled and inoculated, returns to health if it is again warmed. Is not this new receptivity, created by a simple chilling, an interesting fact? Does it not give the explanation of the rôle of circumstances, to all appearances harmless, in the appearance of a disease?

In spite of all these labors followed in the laboratory, Pasteur still finds time to go to the hospital to collect material for new researches. Chamberland and I assisted him in these studies. It was to the Hospital Cochin or to the Maternity that we went most often, transporting into the corridors or into the amphitheater our culture tubes and our sterilized pipettes. One can hardly imagine the repugnance Pasteur had to overcome in order to visit the sick and assist in autopsies. His sensibility was extreme and he suffered morally and physically the pains of others; the stroke of a bistory which opened an abscess made him tremble as if he had received it. The sight of dead bodies, the sad need of autopsies, caused in him a veritable disgust. How many times we have seen him come out of these hospital amphitheaters sick! But his love of science, his curiosity for the truth, were stronger; and he came back the next day.

In the pus of warm abscesses and in that of furuncles a little rounded organism was found arranged in heaps which was easily cultivated in bouillon. It was found in infectious osteomyelitis of children. Pasteur affirmed that the osteomyelitis and the boil are two forms of one disease and that the osteomyelitis is the boil of the bone. In 1878, this assertion caused many surgeons to laugh.

In the puerperal infections, the pus of the uterus, of the peritoneum, and the clots in the veins contain a microbe of rounded

elements arranged in chains. This appears in the form of a rosary, which is especially evident in the cultures. Pasteur did not hesitate to declare that this microscopic organism is the most frequent cause of the infections in lying-in women. One day, during a discussion on puerperal fever at the Academy of Medicine, one of his most distinguished colleagues held forth eloquently on the causes of epidemics in the maternity hospitals. Pasteur interrupted him from his place: "That which causes the epidemic is not at all what you have said; it is the physician and his assistants who carry the microbe from a sick woman to a healthy woman." And as the author replied that he very much feared this microbe would never be found, Pasteur hurried to the blackboard, drew the organism in the form of a chain and said, "That is the way it looks." His conviction was so strong that he could not prevent himself from expressing it forcibly. We can hardly understand to-day the surprise and even the stupefaction in which he put physicians and students when at the hospital, with a simplicity and an assurance which appeared disconcerting in a man who entered for the first time as an assistant at a labor, he criticized the methods of dressing and declared that all the linens ought to be put through the sterilizing oven. In addition to all this, he claimed that he was able to determine by examination of the lochia of the women which ones would have the fever, and he asserted that in the badly infected women he would demonstrate the microbe in blood taken from the finger. And Pasteur did as he said he would. In spite of the tyranny of medical education which then weighed heavily upon men, some pupils were attracted and came to the laboratory to see more closely those methods which resulted in diagnostics so precise and prognostics so certain.

Pasteur never gave botanical names to the microbes which he discovered: he designated them after some peculiarity of their form or of their manner of growth. Thus the microbe of the boil was for him the microbe of clumped grains, that of puerperal fever, the microbe of grains in the form of a rosary. It is these which, under the more regular names of Staphylococcus and of Streptococcus pyogenes, have made such a great highway in bacteriology, as we know it.

Pasteur is again a forerunner when he undertakes with M. Joubert the bacteriological examination of waters which since has led to the saving of so many human lives.

Generally, the infectious diseases do not occur a second time. Is it the same with anthrax? How shall one determine it since it appears certain that every attacked animal soon dies? Nevertheless, among the sheep inoculated in the course of the experi-

ments made at Chartres, some had resisted, while their companions were dead. The idea came that perhaps in the fields of Beauce, so much exposed to anthrax, those sheep which survived had formerly contracted the disease and had recovered from it, this first attack having given them immunity. Some experiments, undertaken by Pasteur and Chamberland, with another purpose in view justified this hypothesis. A veterinarian of Jura had proposed a remedy against anthrax in cattle; to verify the efficacy of it the cattle were inoculated; one half were treated, the other half were kept as controls. In each one of the lots there were cattle which died and others which survived, although they became very ill. Having recovered, these last, without noteworthy disease, endured virulent anthrax inoculation which killed new cattle. The first attack had given them immunity; it is therefore possible to render cattle refractory to anthrax.

This question of immunity dominates the entire history of infectious diseases; Pasteur was led back to it without ceasing by his experiments. He thought of it all the time. The Jenner vaccination was especially the subject of his meditations. What relation is there between the vaccine and the smallpox? Why has Jenner's vaccination remained an isolated fact in medicine? We are ignorant of the nature of the virus of smallpox and of the vaccine virus. But there are viruses which we know better. Would it be possible to find vaccines against them? From our first entrance into his laboratory Pasteur said to us without ceasing, that is, to Chamberland and to me: "We ought to be able to immunize against the infectious diseases the viruses of which we can cultivate." Haunted by this idea, how many impossible experiments have we not gravely discussed, to laugh at them next day, during this laborious period which preceded the discovery of the attenuation of viruses!

The latter was realized in a disease of fowls, the chicken cholera. The microbe of this disease multiplies readily in bouillon made from the muscles of fowls. A young culture is extremely deadly: it kills all the fowls which receive even the least quantity of it under the skin. Kept in a thermostat at 37° C. in contact with the air, this culture loses little by little its activity. After a certain time, inoculated into chickens, it only kills now and then one; at the end of a longer time, it no longer kills them but it makes them ill; finally, it becomes so inoffensive that it produces in them only a slight fever. These fowls, once recovered, will then endure the inoculation of the most virulent virus, mortal for new fowls. It does not kill them, they have acquired immunity.

This experiment therefore realized the artificial attenuation of a virus and preventive vaccination by means of this attenuated virus.

The cause of the attenuation is the prolonged action of air upon the virus, at a suitable temperature. In fact, the same culture which becomes attenuated in the air keeps its virulence in a sealed tube to which air does not have access.

The attenuated viruses, thus prepared, may reproduce themselves for successive generations, transmitting their qualities to their descendants. Attenuation is *hereditary*. The viruses are microscopic plants; they may be modified by culture just like the higher plants. Pasteur has obtained races of virus, as the gardeners obtain races of plants. The methods which gave the vaccine for cholera of chickens have furnished those for anthrax, for rouget of hogs and for still other diseases.

In the preparation of the anthrax vaccine, in the very beginning, difficulty was encountered. The attenuation of the virulence is produced by prolonged action of the air upon the microbial cell. But this cell is modified only so long as it remains in a vegetative state; in this form it is more sensitive to the diverse influences which act upon it. It is not at all the same when the cell forms spores. The agents which modify the vegetative cells have no influence upon the spores, which are much more resistant. The cultures of chicken cholera never give spores; consequently they become attenuated easily. Those of anthrax, which produce spores, remain indefinitely virulent. To attenuate the anthrax bactéridie, it is therefore necessary to prevent them from forming spores. This end was reached by cultivating them at the temperature of 42½ to 43° C.; at this temperature, after the action of oxygen, the attenuation takes place little by little, so that we obtain a series of viruses of diminishing activity by drawing at various times from the culture originally kept at 37° C. These attenuated bactéridie preserve their weakened virulence through successive generations cultivated at 30° to 40° C.; and at this temperature they give once more spores, which fix this virulence. We have thus a whole scale of virus more and more feeble, and which can be reproduced at will. There remains only to choose in this series a culture which, inoculated into animals, gives a slight disease, but one which is sufficient to confer upon them immunity.

The individuals of the same species present very great differences with respect to their resistance to anthrax; consequently, in practice, we make two vaccinations with a twelve days' interval, the first with a feeble virus, the second with a stronger virus, which completes the immunity.

Preventive vaccination against anthrax entered at a bound into practice. The Society of Agriculture of Melun proposed to Pasteur a public trial of the new method. The program was ar-

ranged for the 28th of April, 1881. Chamberland and myself were on our vacations; Pasteur wrote to us to return at once, and when we were together in the laboratory, he told us what had been agreed upon. Twenty-five sheep were to be vaccinated and then inoculated with anthrax at the same time as twenty-five control sheep; the first would resist the disease, the second would die of anthrax. The terms were precise, no place was left for the unexpected. When we remarked that the program was severe, but that nothing more remained than to carry it out as agreed upon, Pasteur added, "What succeeded in fourteen sheep in the laboratory will succeed just as well in fifty at Melun."

The animals were herded at Pouilly-le-Fort, near Melun, on a farm of M. Rossignol, veterinarian, who had proposed the experiment and who was to supervise it. "Especially do not make any mistake in the flasks," said Pasteur gayly, when on the fifth of May we left the laboratory to make the inoculation with the first vaccine.

The second vaccination was performed the seventeenth of May, and each day Chamberland and I went to visit the animals. In these repeated trips from Melun to Pouilly-le-Fort, many observations reached our ears, which showed that the multitude did not believe in our success. Farmers, veterinarians, physicians followed the experiment with a lively interest, some even with passion. In 1881 the science of microbes had very few partisans; many thought that the new doctrines were baleful and regarded it as an unexpected opportunity that they had been able to draw Pasteur and his assistants out of the laboratory to confound them in the open air, in a public experiment. Here was a chance to be done with these compromising novelties in medicine and by a knock-out blow to find again security in the same traditions and the ancient practices menaced for a moment.

In spite of all the passions which arose around them the experiment followed its due course. The trial inoculation was made the thirty-first of May and the second of June was the date set for the return to determine the results. Twenty-four hours before the decisive time, Pasteur, who had gone on with such perfect confidence in the beginning of the public experiment, began to regret his audacity. For some moments his faith was shaken, as if the experimental method might betray him. A too continuous tension of his mind had led to this reaction, which moreover lasted but a short time. The next day, more certain than ever, Pasteur went to confirm the striking success which he had predicted. In the crowd which thronged about him that day at Pouilly-le-Fort, there were no longer any incredulous persons, but only admirers.[3]

[3] The results were perfectly clear-cut; all the vaccinated sheep lived, all the unvaccinated ones died.—E. F. S.

It is now fourteen years since anthrax vaccination has been subjected to practical tests: wherever it has been applied the losses from anthrax have become insignificant. It was followed by a vaccination against measles in swine, in which our poor comrade Thuillier has particularly labored.[3a] But these immediate results are the least merit of the Pasteurian vaccination; they have given an immense confidence in a science which obtained such a success and provoked such an irresistible movement. Especially they have inaugurated that series of researches upon immunity which must finally lead us to an efficacious treatment of infectious diseases.

Virulence is a quality that microbes may lose. They may also acquire it. If we met in nature this anthrax organism attenuated to the point that it would no longer kill any animal, certainly we should not recognize it for the virus of anthrax. It would appear to us to be a saprophytic microbe. One must have assisted in all the phases of its attenuation to know that this inoffensive bacillus is the descendant of a redoubtable virus. We may nevertheless restore to it the virulence which it has lost by inoculating it first into an extremely susceptible being, a mouse only one day old. Cultivated in the body of this very young mouse, the bacteridium recovers its parasitic aptitude. If with the blood of this mouse we inoculate another a little older it will perish. By passing thus from younger to older mice, we finally succeed in killing adult mice, guinea pigs, then rabbits, then sheep, etc.

During these passages the virulence has gone on increasing. This increase of virulence, which we obtain experimentally, takes place without doubt in nature and we can imagine very well that an ordinary microbe harmless for an animal species may become deadly to it. Is it not in this manner that in the course of the ages the infectious diseases have appeared?

From these modifiable viruses which are so plastic that the experimenter, so to speak, shapes them to his will, it is a far cry to the ancient conception of virulent entities! Pasteur's note on the attenuation of the viruses and the return to virulence was presented to the Academy of Sciences the twenty-eighth of February, 1881. Better than any other it gives the measure of Pasteur and makes us understand the extraordinary penetration of his mind.

The researches on anthrax did not occupy all Pasteur's time. During the same period he began studies upon rabies. This disease is one of those which take the fewest victims among men; if Pasteur chose it as the subject of studies it was because the virus of rabies has always been regarded as the most subtle and the most mysterious of viruses, and also because hydrophobia is for every-

[3a] T. lost his life studying cholera in Egypt.—E. F. S.

body the most dreaded and frightful disease. Pasteur shared the common horror; and believed that to solve the question of rabies would be a benefit to humanity and a striking triumph for his doctrines.

In 1880 the first experiments were undertaken. Rabies is inoculable into animals, it is therefore accessible to experimentation; no doubt it is caused by a microbe, and he did not question that the methods which had led to the discovery of so many other viruses would succeed also in demonstrating that of rabies. But it was not so: not only have we never been able to cultivate the microbe of rabies, but no one has succeeded in seeing it. The most patient investigations with the microscope, the most advanced methods of staining have miscarried up to this time.[4] It was necessary to work upon a virus which could not be cultivated and was invisible, and nevertheless we have succeeded in establishing a prophylaxy for rabies, after the bite, the results of which exceed by far the most successful that have ever been obtained in medicine. Is there a more striking example of the power of the experimental method applied to things medical? Rabies is transmitted by the bite of a mad animal, because the virus is contained in the saliva. But inoculations made with the saliva do not give the disease with any certainty; even when they succeed, the latter appears only after an incubation period which is often prolonged during several months. The first step was to learn how to give rabies with certainty; in order to do that it was necessary to give up inoculation with saliva, which, in addition to the rabic virus, contains a large number of common microbes, sometimes hindering its action. Where then should one find in a rabic animal the virus in a state of purity? Rabies is manifestly a disease of the nervous system; perhaps the virus is in the nervous centers. In fact, experiment shows that the true seat of the rabic virus is the brain and the spinal cord. The inoculation of the substance of these organs, taken from a rabid animal, gives the disease with more certainty than the saliva, because the virus is there more abundant, and especially because it exists there in a state of purity. Nevertheless, it is not sufficient to have a pure rabic virus in order to give the disease immediately. The inoculation of the substance of the rabic brain under the skin is not always followed by rabies, and when the latter does occur, it appears most often only after a long incubation period. Subcutaneous inoculation therefore is not to be trusted. Then the idea came that to transmit rabies with certainty the virus should be deposited in the nervous centers, since it is there that it grows best. It was therefore decided that a dog should be inoculated under the dura mater by trepanning the skull.

[4] Rabies is now generally believed to be due to the Negri bodies.—E. F. S.

Ordinarily as soon as an experiment was conceived and discussed it was set in operation without delay. This one, upon which we nevertheless counted so much, was not at once executed; Pasteur, who must sacrifice so many animals in the course of his beneficent studies, experienced a strong repugnance to vivisection. He assisted without too much squeamishness at a simple operation like a subcutaneous inoculation, and yet, if the animal cried a little, Pasteur was immediately full of pity and gave to the victim consolations and encouragements which would have appeared comic if they had not been touching.

The thought that it was necessary to bore through the skull of a dog was disagreeable to him. He earnestly wished that the experiment might be realized, but he feared to see it undertaken. I did it one day when he was absent. The following day, when I reported to him that the intracranial inoculation did not offer any difficulty, he was full of pity for the dog: "Poor beast, its brain without doubt is injured, it must be paralyzed." Without replying I descended to the basement to find the animal and brought it into the laboratory. Pasteur did not love dogs; but when he saw this one full of vivacity nosing about curiously he expressed the liveliest satisfaction and lavished upon it the most amiable words. Pasteur felt a great tenderness for this dog who had so well endured trepanation and had thus removed all his scruples against future trepanations.

This first trepanned dog took rabies characteristically in fourteen days. The experiment, repeated a great many times, gave the same result; it was possible therefore to give rabies with certainty and in a relatively short time; consequently it was easy to experiment with it.

The inoculation of rabic virus by trepanation succeeded also in the rabbit, and it is easy to transmit rabies in this manner from rabbit to rabbit. During these successive passages the virus is strengthened and the duration of the incubation diminished to a period of only six days. In this way we obtain veritable intracranial cultures of the rabic virus. Instead of making the culture in artificial media, as in case of other viruses, we make that of the rabic virus in living material. These cultures in the substance of the nerves may be modified like the cultures of anthrax or of the chicken cholera.

The rabic spinal columns, exposed to the action of the air, in an atmosphere deprived of moisture, dry out and lose their activity. After fourteen days the virus is weakened to that degree that it is inoffensive even in the strongest doses.

A dog which receives this spinal cord fourteen days old, then on the next day a cord thirteen days old, then that twelve days old,

and so on till he receives a fresh cord, does not contract rabies, but he has become refractory to it. Inoculated in the eye or in the brain with the strongest virus he remains well. It is therefore possible, in fifteen days, to give immunity to an animal against rabies. Now men bitten by mad dogs do not ordinarily contract rabies sooner than a month and often more after the bite. This incubation period may be utilized to render the bitten person refractory to the disease.

Experiments made upon bitten dogs or inoculated ones succeeded beyond expectation. You will remember how, with the aid of MM. Vulpian and Grancher, it was extended to man. More than sixteen thousand persons have undergone to-day antirabic treatment and the mortality of these treated persons has been less than one half of one per cent.

The discovery of the prophylaxy of rabies brought about everywhere a veritable enthusiasm. It did more for the popularity of Pasteur than all his previous labors. In return for such a benefit, the great public wished to manifest its gratitude in a fashion worthy of itself and of him who was the object of it; it was then that a subscription was opened which laid the foundations of the Pasteur Institute.

It seems that those results acquired in the study of rabies might have presented themselves naturally to the experimenter and in a logical order. One must have participated in this study to know what stubborn labor it necessitated during more than five years. Pasteur displayed therein that tenacity of purpose which leads to success. How many times, in the presence of unexpected difficulties when we could not imagine any way out, I have heard Pasteur say: "Let us do over the experiment, the essential thing is not to be discouraged."

After the studies on rabies, the health of Pasteur continued to decline. He supported better the obstinate labor of the period of researches than the emotions of the triumph. Pasteur welcomed gladly the demonstrations and the universal gratitude lavished on him, not through a vain love of praise, but on account of the honor which accrued from them to his country, to science and to his loved ones.

The numberless manifestations of which he was the object at this period excited his sensibility even to tears. As soon as the preventive inoculations were applied to man all repose was lost for him. Every bitten person brought him a new preoccupation. The sight of wounded children especially caused him emotions which he could not control. When desperate cases supervened against which no method was of any avail, Pasteur suffered all the torments of

these diseased ones. Every visit he made to them was for him a torture, and he could not help visiting them. It was necessary to get him away from Paris. He was in Italy when those attacks appeared against the antirabic method which made so much noise at the epoch and which are so thoroughly forgotten to-day. He felt them at a distance and was keenly afflicted by them. From this time, Pasteur had to renounce the life of the laboratory; for such a worker as he was, the inaction was a sorrow. Only the visits of his collaborators and the company of his grandchildren were capable of restoring to him a little gaiety. The never-to-be-forgotten ceremony of his Jubilee, in December, 1892, by showing him what a place he held in the esteem of scientific men and in the veneration of peoples, caused him to experience a profound emotion. From that time on Pasteur lived only in the love of his own people; all the care and all the affection with which he was surrounded was necessary to keep him alive, he was so feeble. But even to the end, his thought was in the laboratories, with those who labored strenuously in order that the foundation which bears his name might remain worthy of him. These labors on anthrax, the attenuation of virus, the swine fever, rabies, Pasteur accomplished them in less than ten years, from 1876 to 1885, with the aid of only a few collaborators, M. Joubert at first, and then MM. Chamberland, Thuillier and Roux. Those years passed in the laboratory of rue d'Ulm during this period of discoveries remain present in my mind as the best ones of my life. In order to be nearer the work, master and disciples lived in l'École Normale. Pasteur was always the first to arrive; every morning, at 8 o'clock, I heard his hasty step, a little trailing [on account of his paralysis] over the loose pavement in front of the room which I occupied at the extremity of the laboratory. As soon as he had entered, a bit of paper and a pencil in his hand, he went to the thermostat to take note of the state of the cultures and descended to the basement to see the experimental animals. Then we made autopsies, cultures and the microscopic examinations. One must have seen Pasteur at his microscope to have an idea of the patience with which he examined a preparation. Moreover, he looked at everything with the same minute care; nothing escaped his myopic eye, and we said jokingly that he saw the microbes growing in the bouillons. Then Pasteur wrote out what had just been observed. He left to no one the care of keeping the experimental records; he set down most of the data which we gave him in all its details. How many pages he has thus covered, with his little, irregular, close-pressed handwriting, with drawings on the margin and references, all mixed up, difficult to read for those not accustomed to it, but kept nevertheless with extreme care. Nothing was

set down which had not been established; once things were written, they became for Pasteur incontestable verities. When, in our discussions, this argument resounded, "It is in the record book," none of us dared to reply. The notes being taken, we agreed upon the experiments to be made; Pasteur stood at his desk ready to write what should be decided upon, Chamberland and I facing him, with our backs to a show-case (vitrine). It was the important moment of the day; each one gave his opinion, and often an idea, confused at first, became clear in the course of the discussion and ended by leading to one of those experiments which dissipated doubts. Sometimes we were not in agreement and the discussion was heated, but, with Pasteur, who passed nevertheless for the authority, one could say freely all his thought; I have never known him to resist a good reason.

A little before noon, they came to call Pasteur for the midday meal; from noon till two o'clock he came into the laboratory and the most often, on our return, we would find him motionless in front of a cage, never wearying of observing a guinea pig or an interesting rabbit. About two o'clock Madame Pasteur sent for him, else he would have forgotten to go to the Academy and to the committees of which he was a member. Then we spent the afternoon in making the experiments agreed upon, interrupting ourselves only in order to allow Chamberland to smoke a pipe. The master had a horror of tobacco and we smoked only in his absence. Pasteur returned toward five o'clock. He informed himself immediately of all that had been done and took notes; his notebook in hand, he went to verify the tickets fastened on the cages, then he told us the interesting communications heard at the academy and talked of the experiments in progress. It was at this moment that Pasteur revealed his thought most willingly, especially if we provoked him by objections; then his clear eye flashed more vividly; his speech, a little thick in the beginning, became more and more animated and persuasive. He developed the most profound and the most unexpected ideas, he proposed the most audacious experiments. This rigorous experimenter had a powerful imagination; for him *a priori* there was nothing absurd. But his most enthusiastic flights always led him to an experiment to be made and he retained only that which could be demonstrated. His ardor was so communicative that after having heard him the experimental projects filled the mind. When we got him going on the subject of his first labors, he expressed himself like a poet on molecular dissymmetry and its relation to the dissymmetric forces of nature. Those days, Pasteur forgot the dinner hour; and Madame Pasteur had to send for him two or three times, or come to find him herself; then he went away smiling and saying to us, "You are to blame, I shall be scolded."

The laboratory was always shut up; one was able to enter it only after having sounded at the great door, constantly closed. Visitors rarely got past the antechamber; when Pasteur was at work he was not receptive, even to his friends; to interrupt him was to make him unhappy. I see him turning toward the intruder, waving his hand as if to send him away, and saying in a supplicating and desperate tone: "No, not now, I am too busy." He was nevertheless the most simple and the most approachable of men, but he did not understand how any one could disturb a scientific man who is occupied in making laboratory notes. When Chamberland and I were pursuing an interesting experiment, he stood guard over us. Seeing at a distance, through the glass windows, the comrades who came to ask for us, he went himself to receive them and to turn them away. These whims of Pasteur showed so naïvely his sole preoccupation in the work that they never vexed anybody.

Pasteur has been reproached for not having opened widely his laboratory to students who might have spread his ideas and his methods; it has even been said that he loved to keep secret his methods of research. Nothing is more unjust; in his communications, Pasteur sowed with an open hand new ideas and every one has been able to profit by them. He was therefore an incomparable teacher; he has not advanced any idea without giving information enabling any one to control it; but instead of losing himself in useless details and explaining clumsily the arrangement of apparatus which everybody could easily imagine, he confined himself to stating exactly the necessary and sufficient conditions. The laboratory of the rue d'Ulm was not large enough to receive numerous investigators, there was just enough room for Pasteur and his assistants.[5] Moreover, Pasteur worked easily only in silence and retirement; near him he admitted only his collaborators; the presence of a stranger to his occupations sufficed to disturb his work. One day when we went to see Wurtz at l'École de médecine, we found the great chemist in the midst of his students, in his laboratory full of activity, buzzing like a beehive.

"How can you work," said Pasteur, "in the midst of such a disturbance?"

"That only stirs up my ideas," replied Wurtz.

"That would drive all mine away," replied Pasteur.

Pasteur was constantly imagining new experiments; he noted his projects upon sheets of a little notebook or upon morsels of card-

[5] In the laboratory reserved for *l'agrégé préparateur*, several persons have been admitted to work, notably M. Denys Cochin. Later, when the annex in the Rue Vauquelin was fitted up, Pasteur hastened to receive there Dr. Straus and then Dr. Grancher.

board which he carefully preserved. His left hand remaining useless after the paralytic attack of 1868, he confided the execution of experiments to his assistants; an irreproachable experimenter himself, he showed himself very exacting toward others. For him, there was no impossible experiment: when we observed that that which he demanded of us presented very especial difficulties he said, "That is your affair; do it in any way that seems best to you, provided it is well done." And he satisfied himself always that it was well done; he separated out the good from the bad with admirable sagacity.

A communication of Pasteur to the Academy of Sciences or to the Academy of Medicine was an event; because he published nothing that was not finished. Each one of his notes takes up only some pages of the *Comptes rendus,* but it contains the substance of hundreds of experiments. Consequently we can read them and reread them, and always find there something of use; often a simple phrase indicates a new pathway, and several of those which are thus noted have not yet been traversed. All Pasteur is in his writings; his imagination reveals itself therein by the profundity and the audacity of the generalizations, by the rigor of his spirit, by the justice of his views, by the soundness of his conclusions and his enthusiasm by the emotion of his language.

Before writing, Pasteur read and reread the experimental notebooks, then he dictated to one of us or more often to Madame Pasteur. He kept the manuscript sometimes during weeks, retouching it without ceasing; when he was satisfied, he read it to us and discussed with us its phraseology; often he received our comments with impatience; but he always took them into account if they were just. Madame Pasteur recopied it in her fair handwriting, so easy to read; Pasteur would never have sent to the publisher a manuscript full of erasures; if he modified some passage he glued over these lines gummed paper, cut to the proper dimensions, and wrote it anew. During all this work of editing, the question under treatment developed singularly, and we, the collaborators of the master, who knew at what point the experiments had abandoned him, were astonished to see it grow and be transformed in the final note.

The ideas of Pasteur were too new not to be opposed; besides, Pasteur did not fear strife; his discussions at the Academy of Sciences have remained celebrated; those which he maintained at the Academy of Medicine were more passionate still. Many physicians, in fact, and not the least conspicuous ones, saw at first with stupefaction and then with indignation this chemist overthrow with so much assurance medical doctrines. To study diseases in a laboratory with chemical apparatus was for them a medical con-

tradiction. On his part, Pasteur, persuaded that he brought the truth, would have believed it a bad action if he had not maintained his point with all his force. Hence those contests with which the heroic ages of bacteriology have resounded; every discovery of Pasteur has been imposed with blows; when he despaired of convincing his colleagues, he addressed himself over their heads to the public of young physicians who followed the lectures.

Under these contradictions he lost his serenity, and as he was sure of the propositions he advanced, he proposed willingly the nomination of academic commissions before whom he would bring his adversaries as before a tribunal.

So much courage and stubbornness rallied partisans to his doctrine, but there remained irreductible opponents who came back without ceasing to the charge. There is nothing astonishing in the fact that Pasteur and they could not understand each other. They were imbued with that medical spirit made up at the same time of skepticism and of respect for traditions. He had the faith of a novice and the assurance which the experimental method gives. He was indignant that any one could remain hesitant in the face of a demonstrative experiment.

He left these seances stirred to the depths; MM. Vallery-Radot, Chamberland and I often awaited him at the exit: "Have you heard?" he would say to us: "To the experiments they have replied with talk!" We returned on foot to the rue d'Ulm and his irritation gradually subsided; immediately he spoke of making still more experiments to throw additional light, for the contradictions excited him to make new researches. These tumultuous seances at the Academy of Medicine were therefore useful, since they were like a stimulant to the activity of Pasteur.

The passion of Pasteur for science carried him away sometimes in outbursts very amusingly naïve. For him, a man who made a bad experiment or a false reasoning was capable of anything. One day when he read to us in the laboratory a work which appeared to him to be particularly bad, he was exasperated and cried out: "A man who can write such stuff I would not be surprised if he should beat his wife," as if to beat his wife were the height of scientific irregularity!

The great force of Pasteur is that he could, without being weary, hold his thought concentrated upon the same object. He followed his idea without allowing himself to be distracted and he brought everything to it; thus from a conversation with persons even the most unfamiliar with science he knew how to draw something useful for his researches. Of him also one can say that he has made

his discoveries by continually reflecting on them. His stubborn thought attached itself to difficulties and ended by solving them as the intense flame of the blowpipe constantly directed upon a refractory body ends by melting it.

In those moments of great preoccupation, Pasteur remained silent, even in the midst of his own family. Nothing could efface the obstinate wrinkles of his countenance until the solution was found. Then his face became luminous, and this concentrated man allowed his joy to overflow, explaining what he had discovered and what he hoped from it. All those close to Pasteur and associated with his scientific life felt the rebound of his preoccupations and participated in the satisfactions of the savant.

One can not understand the career of Pasteur if he does not know of his family and especially of Madame Pasteur. From the first days of their common life Madame Pasteur understood what sort of a man she had married; she strove to remove difficulties from his life, taking upon herself the cares of the household in order that he might keep all the liberty of his spirit for his researches. Madame Pasteur has loved her husband to the point of understanding his work. In the evening she wrote under his dictation and brought out explanations, for she was really interested in the hemihedral facets and in the attenuated viruses. And then she had clearly perceived that ideas become clearer when they are explained and that nothing leads more certainly to the conception of new experiments than the description of those which have been made. Madame Pasteur was not only an incomparable companion for Pasteur—she was his best collaborator.

The work of Pasteur is admirable. It shows his genius, but one must have lived intimately with the master to know of the goodness of his heart.

BIOLOGICAL STATIONS FOR THE STUDY OF PLANTS AND ANIMALS TOGETHER

By WALTER P. TAYLOR

BIOLOGIST, U. S. BUREAU OF BIOLOGICAL SURVEY

THE world of nature is a unit. If it is upset or interfered with in one place the entire system is bound to be affected. Before man's appearance the animals and plants had undoubtedly attained a degree of equilibrium. But such human activities as reclamation of arid lands, grazing and lumbering have thrown the natural system off-balance, leading to unforeseen consequences in many directions which must be studied and controlled if man is to maintain himself and his civilization on anything like the present basis. As Lankester[1] puts it, civilized man has proceeded so far in his interference with extra-human nature, has produced for himself and the living organisms associated with him such a special state of things by his rebellion against natural selection and his defiance of nature's pre-human dispositions, that he must either go on and acquire firmer control of the conditions or perish miserably by the vengeance certain to fall on the half-hearted meddler in great affairs. We may indeed compare civilized man, as Lankester points out, to a successful rebel against nature who by every step forward renders himself liable to greater and greater penalties, and so can not afford to pause or fail in a single step.

In no provinces are disturbing consequences more in evidence than in forestry and grazing. Forests have been cut down and range-lands overgrazed with little thought of the future, until, already, the supply of those basic products, meat and wood, is threatened.

While the problems involved are plant problems, of course, there does not seem to have been any very wide appreciation of the fact that they are animal problems, too. A moment's review will, I think, show that investigations of animals no less than plants will be necessary to the accomplishments in forestry and grazing which are of such vital importance to the people. Among the animals of importance in this connection are, of course, representatives of various groups, ranging, doubtless, from the protozoa to man. I am limiting the present brief exposition principally to the mammals and especially the rodents.

[1] "Kingdom of Man," 1911, pp. 31–32.

Relations of Rodents and Grazing

Something is known as to the effect of the banner-tailed kangaroo rat and of the Zuni prairie dog on vegetation. The kangaroo rat has been found to be a champion hoarder, and stored food, principally the seeds and crowns of important forage grasses, and varying in amount from one sixth of an ounce to more than twelve and one half pounds, has been found in different dens. In one case approximately 680,000 separate cut sections of grass had been stored by this rodent, showing its extraordinary activity. The ground for a radius of 15 to 25 feet about the den may be practically denuded of vegetation. During periods of extreme drought the species may be of critical importance on grazing areas from the standpoint of carrying capacity of the range.

Studies by the Biological Survey and its cooperators of prairie dogs and range grasses in northern Arizona have shown that prairie dogs on certain areas consume a large proportion of the forage. Furthermore, these rodents are more destructive to grasses than are cattle, for they can crop more closely. In the absence of thoroughgoing control measures the prairie dog will undoubtedly be a strong factor in the ultimate destruction of all forage on vast areas of good stock range.

Some of these desert rodents, notably the kangaroo rat and pocket mouse, can go all their lives, apparently, without drinking water. The suggestion has recently been made that as the world dries up, mankind will ultimately be dependent on jerboas, jack rabbits and other drought-loving species. While this suggestion at present seems fantastic, we ought, as a matter of fact, to know a good deal more of the ecology and physiology of these rodents than we do.

Work already done on certain desert rodents is suggestive. We know very little of a definite and satisfactory character of the relation of jack rabbits, wood rats, pocket mice, grasshopper mice and various ground squirrels to grazing. Some may be harmful, some beneficial, and some neutral, in relation to the forage plants and valuable shrubs of the desert. All the burrowing rodents, even the pest species, may subserve an important and beneficial function in cultivating the soil and making it fit for the growth of plants. Man must of course protect the forage from pests, but he must have more facts regarding animals, their life histories and their relation to their surroundings, if he would avoid pitfalls in the application of control measures.

Rodents and the Forest

We in the United States are using up our forests more than four times as fast as they are being grown. The virtual end of our wood supply is in sight if we do not immediately increase our forest area and the rate of forest production. We can do this by keeping fire out of the forest, growing trees on the eighty-one million acres of land which were formerly forested but which are now a desolate waste, and by adequately maintaining the reproduction and growth of timber in such forests as remain.

In either artificial or natural reproduction of forest the rodent problem is a serious one. At the Wind River nursery[2] it was found that one mouse could eat three hundred seeds of the Douglas fir, and one chipmunk six hundred, in a single day. If 42,350 seeds were sown on an acre, ninety mice could consume the bulk of the seeds in one night and have only enough left for a light lunch the next night. In Arizona and New Mexico we have one of the largest continuous forests of western yellow pine in the west. Investigations at the Southwestern Forest Experiment Station, near Flagstaff, have shown that rodents are an important factor in yellow pine reproduction under natural conditions, and become even more important on cutover lands.

Nor is the case much better with planting, that is, the setting out of young trees raised in the nursery. The principal rodent offenders in connection with planting projects in the west appear to be rabbits, wood rats and pocket gophers. Rabbits girdle the young trees and sometimes eat the foliage. Wood rats cut off the young trees and carry them to their nests. Pocket gophers cut off the roots.

The actual or possible benefits of some rodent activities should not be overlooked. Quite often the rodent which eats tree seed scatters and plants it. Squirrel caches are recognized sources of pine and redwood seed. Hoffman says that the tree squirrel of the northwest is a furred forester of much importance and value, playing a leading part in the reproduction of the Douglas fir. During the summer of 1919, following the great Cispus burn of 1918, he found stands of up to 40,000 seedlings per acre. These little trees were from seeds buried in the forest floor by the squirrels and surviving the fire. Certain rodents may fill an important place in the natural economy of the forest as soil-makers and cultivators, as consumers of vegetation inimical to trees, as natural pruners of overfull tree crowns, or even as automatic thinners of overthick stands.

[2] Willis, C. P. "The control of rodents in field seeding," Proc. Soc. Amer. For., IX, pp. 365–379, 1914.

Forest scientists have been trying for some years to determine the factors limiting the distribution of forest trees. Results of the work of English ecologists clearly suggest that in some cases rodents may be a more critical factor in limiting forest distribution than climate or soil.

The importance of the study of forest rodents has been recognized by a number of leading biologists and foresters. The species of rodents in different localities are often not the same. The habits of each species are to a great extent peculiar, and the traits of the same animal may be unlike at different times of year. In one case a thorough poisoning in Montana was ineffective because the grain was distributed in hoarding season and stored without being eaten.

Conclusion

Thorough studies of plant ecology and of plants, their habits, life histories, acclimatization and uses are all too rare; but adequate provision for attention to problems on the animal side is lacking. The agricultural stations, colleges and universities are doing all too little on this head. The Carnegie Institution of Washington, while it is doing much work with plants, is not engaged in animal investigations to any extent. The government bureaus, notably the Biological Survey and the Forest Service, are apparently doing more, at present, than any other agencies, but their work is incomplete.

This lack of attention to zooecology, or perhaps better, bioecology, is the more to be deplored because the problems of culture, maintenance and administration of agriculture in general and forage and forest in particular so directly involve animals as well as plants. Many questions which arise are essentially biological rather than botanical or zoological alone.

Much of our practice in grazing, forestry and animal administration is still on a basis of trial and error, when it ought to be scientifically grounded. Adequate provision through existing scientific organizations or new ones for the study of plants and animals together would be of extraordinary value both to the advancement of biological science and the welfare of the people.

RADIO TALKS ON SCIENCE[1]

SPONTANEOUS COMBUSTION

By Dr. CHARLES E. MUNROE
NATIONAL RESEARCH COUNCIL

WHEN you light gas it ignites and burns because coal gas is inflammable and combustible and because it comes into contact with and becomes mixed with air, which surrounds the gas burner. Analysis shows that air is a mixture of many gases, the principal ones being oxygen and nitrogen, but the oxygen is the one of them which reacts with the gas to bring about the burning. All ordinary combustion is due to a similar chemical reaction between a combustible substance and oxygen.

It is familiar to all that kindling wood ignites more easily and burns more readily than the logs from which the kindlings have been cut, and that shavings of this same wood ignite still more easily and burn still faster. If we throw the sawdust from such wood into the air so as to produce an intimate mixture of the two, on ignition the combustion will be so rapid that an explosion may result. All finely divided combustibles such as dusts, vapors and gases form dangerous mixtures with air.

The readiness with which substances burn and the fierceness of the fire is the greater the richer the air is in oxygen; and, when pure oxygen is made use of, iron and many other substances usually regarded as incombustible burn readily in contact with it. By the use of oxygen combustion may go on under water. An experiment frequently shown in chemistry lectures is performed by partly filling a conical wine-glass with warm water, dropping into it a bit of phosphorus which will drop to the bottom and melt, and then, by means of a glass tube attached by a rubber tube to a cylinder containing compressed oxygen, leading the oxygen into contact with the phosphorus when the latter will burn brightly under and in contact with the water.

In lighting gas or the paper or shavings used in starting a fire, the match is used to heat them up to the temperature at which they will react with the oxygen in the air in which they are immersed. To produce fires it is necessary to have present a combustible, a

[1] Broadcast from Station WCAP, Washington, D. C., under the auspices of the National Research Council and Science Service and the direction of Mr. W. E. Tisdale.

supporter of combustion, such as oxygen is, in contact with it, and that they shall be heated up to the temperature at which the action begins. Each is equally essential.

There are many ways in which bodies may be heated besides by a burning body, such as a match. Friction supplies one way, and long before matches were invented our ancestors started a fire by the friction of two sticks twisted against one another. Later, they ignited gunpowder in a flint-lock musket by the friction of flint on steel, which struck a spark. More than a century ago Dobereiner demonstrated that when gases came in contact with porous solids the gases were condensed within the solids and heat produced to such an extent that if the gas was a mixture of combustible gas and air or the solid was combustible and the gas air, the combustible took fire and burned. Fermentation, due to bacterial action, also causes bodies in which it is taking place to heat up. There are still other natural agencies continually operating which may cause combustible bodies in contact with air to heat up to the temperature at which they take fire and burn. It is fires originating under such conditions that are designated as having been caused by spontaneous combustion.

It is well known that fires are of frequent occurrence in paint shops, and until recent times they were looked upon as of mysterious and perhaps supernatural origin, as they broke out when no one was about and under circumstances which did not warrant suspicion of their having been deliberately started. The chemist has found the cause to lie in the presence in paint shops of oil and particularly linseed oil which, when exposed to the air, greedily takes up oxygen from it and heats up. This occurs the more readily if this oil drops upon the porous lampblack, also much used by painters, for then, owing to the operation of the Dobereiner effect, the mixture rapidly heats up to its point of ignition and burns.

Other combustibles, such as cotton waste, rags and the like, which contain oil, behave in a similar manner, though they may not ignite so promptly; many a house has been set on fire and destroyed by oily rags or waste on which the painter has wiped his hands, or that has been used in oiling the floor. *Beware of oily rags!* Keep them during the day, when not in actual use, in a tinned can or bucket, and burn them up, in a safe place, at the end of the day's work.

Charcoal, in lumps, is so porous that it may, unless carefully cooled and slowly aerated as it comes out of the kiln or retort in which the wood is charred, absorb and condense air within its pores so fast as to burst into active combustion. Charcoal fires in railway cars, on which it is being transported, or bins in which it was stored

have been numerous. When a steam coil is placed very close to a wooden wall the wood may in time become very dry and porous, and sufficiently so to absorb and condense air within its pores and to break out into flame. Many fires in buildings have started in this way. Make a survey of your heaters.

Though most of us nowadays buy our bread from the bakers, yet pretty nearly everybody has some knowledge of fermentation, which is taking place in nature all about us when the proper conditions of fermentable materials (such as starch, sugar or cellulose) and moisture and temperature exist together. A proper initial temperature to start the reaction is as essential in fermentation as it is in lighting the gas or shavings. When but a moderate amount of moisture is present the heat evolved in fermentation may raise the temperature of the pile to such a degree that the fermenting material takes fire and burns. This has frequently occurred with brewers' grains, castor pumace, new-mown hay and similar organic substances. The cut grass is spread in converting it into hay in part to avoid this fermentation. Many a barn has been burned because, owing often to inclement weather, hay has been stored in the loft before this hay was fully cured.

You have noticed, particularly on dry, cold days in winter, that, as you walked across the rug, when you were wearing carpet slippers, or in your stockinged feet, you acquired on your body an electric charge and that as you touched a radiator, or faucet, a spark was emitted from your finger. Some persons have acquired, in this way, so considerable a static charge as to be able to light the gas by the spark from their finger. Moving belts, driving machinery, and other parts of the mechanism acquire quite considerable static charges and these have often caused serious fires in grain elevators, rubber mills, explosives factories, cereal works and other industrial establishments where inflammable materials and especially dusts and vapors existed. To play safe electrically, ground the machinery and keep the place free from combustible dusts, vapors and gases.

Objects moving through the air, tanks containing an oscillating liquid, such as gasoline, when the tanks are mounted on rubber-tired trucks, cylinders holding compressed gases, when the contents issue through small openings as a mist, all acquire very considerable static charges and the sparks attending the discharge have been the initiating cause of the destruction of many hydrogen-filled dirigibles. The authorities in our army and navy have wisely decided that our dirigibles shall be filled with incombustible helium.

The menace of the static charges on gasoline tank trucks, which led to many fires at filling stations, has been overcome by attaching

to the tank a chain long enough to drag on the earth, thus "grounding" the tank.

Compressed and liquefied oxygen is meeting with larger and larger use in oxy-acetylene and other blow-torches, in explosives and for other purposes; and it is proposed to operate blast furnaces with this element. It is obtained by liquefying air and, through fractional distillation, separating the liquid into its components. Compressed oxygen is put into strong steel cylinders under pressures of 1,800 to 2,000 pounds per square inch. Naturally accurately fitted valves are required to confine the gas and liberate it as wished. There is a temptation to put oil on these valves to make them work more easily, but don't you do so, for through the fire and explosion that may follow you may be destroyed. Because of the many accidents from this cause Professor Hersey and his associates have, for nearly two years, made this matter a subject of experimental investigation at the Bureau of Mines Experiment Station, Pittsburgh, and he published an article entitled "A study of the oxygen-oil explosion hazard" in the *Journal of the American Society of Naval Engineers* for May, last. As you value your life and property, protect them by implicitly following the "practical safety precautions" set forth by Professor Hersey whenever you fill, handle, store, transport or use cylinders containing compressed gases.

GLASS

By H. E. HOWE

INDUSTRIAL AND ENGINEERING CHEMISTRY

It is doubtful whether any of man's inventions mean more to the race than the chemical compound glass. The everyday uses of glass are numerous, both in the home and in every industry. It is indispensable to the scientist who without the microscope and other optical instruments could not have performed his work in adding an average of sixteen years to the life of men within the last two generations. To glass also we owe those aids to vision which are beyond price and those great discoveries impossible without lenses for astronomy and photography.

Glass is a chemical compound of which there are a great many varieties. The differences between some of these are so slight as to require intricate instruments to distinguish them. The various classes of glass are distinct, and it is seldom that glass of one class can be used for the same purpose as that of another. Thus the most excellent plate glass is not satisfactory for optical purposes.

Glass is composed of such compounds as silica or sand, soda ash, potash, lime and the oxides of lead, to name only the more common ones. To impart color to glass, gold is used for ruby, selenium for pale rose, copper for red, silver for yellow, carbon for brown, cobalt for blue, nickel for purple and chromium for green colors. Sulfur gives shades from a greenish yellow to black, depending upon the materials in the glass with which it can combine to produce pigment materials.

Formerly all glass was made by the handblown process, but today much of it is made by machinery, thanks to the contributions of the mechanical engineer, the physicist and the chemist, the latter having so improved the glass composition as to give it the mechanical strength necessary for these operations. The glass blower who formerly worked on short shifts before the furnace, swinging in a pit a heavy charge of glass gathered on his blow pipe from the molten mass, has been replaced by the glass-blowing machine. One type of machine draws up great cylinders of glass directly from the tank furnace and from these cylinders our window glass is made. Another machine gathers the molten glass, molds it into table tumblers, finishes these, anneals them and delivers them to the packing cases at the rate of 20,000 a day. Still another type of machine produces glass tubing or glass rod by pulling the glass under constant speed from the tank furnace, air being used if tubing is the product, while without the air glass rods of practically any length are manufactured.

Bottles, electric lamp bulbs and similar articles of quantity production are now machine blown. Whereas window glass is still made from machine-drawn cylinders, a machine which will produce sheet glass directly from the furnace is being developed. In all this work the invention of the mechanical engineer awaited the results of the chemical laboratory before their perfection.

It is in optical glass that the greatest progress from the standpoint of science has been made in recent years. New discoveries in astronomy, biology, physics and chemistry followed the perfection of new types of optical glass late in the nineteenth century. Prior to 1914 there had been no optical glass and very little chemical glassware made in America. The relatively small market, the difficulty of manufacture, the lower costs abroad, all had contributed to the willingness to purchase these specialties in Europe. A wonderful record in the production of other varieties of glass had been made, and the mechanical development in glass manufacture had been carried further in America than elsewhere. Then the time came suddenly when these particular glasses were all-important for telescopes, field glasses, range finders and other military equipment

requiring the finest optical systems, as well as chemical glassware necessary in the control of industrial processes. America called upon her scientists to develop for her while she waited an optical glass industry such as had been built up during thirty years of European experience.

It is notable that the success which attended these efforts was due largely to information acquired by scientists working in the field of pure research, rather than to those experienced in glass manufacture. Important contributions were made by the Geophysical Laboratory of the Carnegie Institution of Washington, where specialists in the silicates were able to apply their knowledge so as to overcome great difficulties, and by the Bureau of Standards of the Department of Commerce, which contributed invaluable service in developing new types of pots in which the optical glass batch is melted. The Geological Survey performed its part in locating necessary deposits of exceptionally pure sand, special clays and other required raw materials. By correlating available information on the physical characteristics and chemical composition of glasses, it was possible to prepare after one or two trial melts large quantities of glass to the exacting specifications of the mathematical optician. Within a short time America was able to supply her own optical glass and a surplus for her allies.

Concurrently chemical and scientific glassware for laboratory and plant use was developed to a high degree of perfection and continues to be supplied not only for that type of work but for household purposes and more lately as chemical engineering equipment for chemical plants.

For many, the most important glass is that supplied in spectacle lenses. The old practice was to blow large balloons with heavy walls and then cut from the surface a number of rough lenses to be ground and polished. More recently this glass has been made in sheets in much the same fashion as window glass. The oval or round blanks are cut out and pressed roughly to shape before grinding and polishing to accurate curves. This grinding is done with successively finer grades of emery or corundum, and the lens is polished to a brilliant surface with oxides of iron. During the various operations the lenses are held in place by a mixture of pitch and rosin. The edges are ground on carborundum stones, and if cemented as in the old type of bifocal, the gum from the Canadian balsam tree is the favorite cement. The invisible bifocal made by fusing together in the electric furnace two kinds of glass, the curved surfaces of which are previously ground and polished, is a valuable contribution of the last few years. One-piece bifocals, that is, a

single lens with one part focused for reading and one part focused for distant vision, have also been prepared commercially.

The properties of all glass depend upon those of silica and the silicates, borates and phosphates, all chemical compounds which are formed in the production of glass. New characteristics depend upon new compositions. Having learned the requirements, scientists continue in their effort to improve glass, and should they be as successful as were the investigators between 1880 and 1900, there might follow another series of advances in the important sciences which would open up for the benefit of the race a corresponding new series of discoveries and inventions.

MEASUREMENTS OF THE TEMPERATURE OF MARS

By Dr. W. W. COBLENTZ

U. S. BUREAU OF STANDARDS

In beginning this talk to-night I can not refrain from expressing a feeling of awe and wonderment at the progress made in radio. I wonder how many of you who are listening in realize that the whole subject of wireless telegraphy is but little more than twenty-five years old. In the winter and spring of 1900 a fellow-student, Cartmel, and I transmitted electromagnetic waves from one building to another at Case School of Applied Science, Cleveland, Ohio. The receiving and the sending apparatus was so crude and uncertain in its action that when we were ready to send the wireless waves we first signaled to each other with bicycle lamps. No wonder the local papers came out in large headlines telling how "through a blinding snowstorm" two Case students had sent wireless messages from one building to another.

In the meantime has come all this wonderful development, even to radiotelephony.

Now here I am to-night, using the spoken word, transmitted in electromagnetic waves, across many miles of space, to tell you something about the measurement of the temperature of the planets, especially of Mars.

Whether or not Mars is inhabited and whether or not the Martians attempted to signal to us with bicycle lamps or other means, during the past summer, we do not know. But we do have something definite regarding the temperature of the surface of Mars. Our measurements show that the temperature of the dark areas on

Mars rises above 0° C. This shows that vegetation can exist on Mars. Whether vegetation does exist on Mars is an entirely different question, which depends upon the presence of oxygen.

Mars rotates on its axis once in twenty-four hours and thirty-seven minutes. Hence its day is about one half hour longer than ours. But its seasons are about twice as long as ours. As a result during the long summer season in the polar regions of Mars the temperature rises considerably, just as it does in Alaska and Siberia.

The temperatures of the planets are measured by means of extremely small thermocouples, made of two kinds of wire, the diameter of which is smaller than that of a human hair. The junctures of these two wires are flattened into little disks called receivers, which are about one one hundredth of an inch in diameter. These thermocouples are mounted in the eyepiece of a reflecting telescope. They take the place of the cross-hairs in the eyepiece, and the temperature measurements are made by setting these thermojunctions (instead of the cross-hairs) upon the bright and the dark spots on Mars, also on the craters of the moon. The heat rays emanating from these spots warm the little metal receivers, giving an indication of the temperature of the surface.

You are all familiar with the dark and bright markings on the moon, as viewed without a telescope. These markings have the appearance of the face of the "man in the moon." Similarly, but as viewed through a powerful telescope, the surface of Mars shows bright and dark areas, resembling somewhat the markings on the moon as viewed with the naked eye. Superposed upon these somber markings are the polar caps of Mars, which shine forth as bright as the frosted bulb of an incandescent lamp. But strange to say, these bright polar caps are cold. When the thermocouple receivers are placed upon the image of the polar caps they show no heating because the polar caps do not emit infra-red rays. The polar caps are no doubt composed partly of snow and ice. The observations on the polar caps form some of the most interesting and fascinating parts of the work of measuring the heat radiated from Mars.

It was observed long ago by Schiaparelli and by Sir William Herschel that the polar caps wax and wane, which would be the case if Mars has seasons of winter and summer as we have on this earth.

The radiometric measurements made by Mr. C. O. Lampland and myself at the Lowell Observatory, Flagstaff, Arizona, confirm in a remarkable manner the deductions from the visual observations which have been in progress for years. For example, from the fact

that the glistening white appearance of the equatorial region of Mars is not visible at the Martian noon hours it was concluded that the frost or snow melts and the temperature rises above freezing. Similarly, from the disappearance and reappearance of frost and snow on the bright and the dark areas it was concluded that the bright areas are high plateaus which have a lower temperature than the dark areas.

As viewed with the eye, the dark areas are observed to change in color with changes in the season. In the Martian spring season these areas have much the appearance of the bright areas. As the spring season advances, these dark areas assume a darker appearance and show a more distinct bluish-green color. Later on as the season advances to what corresponds to autumn on this earth the dark markings on Mars take on a more brownish or copperish color. From these changes in configuration and color these dark markings have been interpreted as being caused by the presence of vegetation. Of course you understand that while some astronomers have given this interpretation to their observations others have taken the opposite view and have held to the belief that the temperature of Mars can not rise above the freezing point of water.

Now let us notice the results of our radiometric measurements. Our radiometric measurements show that the bright areas on Mars are cooler than the dark areas. This is just the reverse of conditions on this earth, where the surface of the bare desert areas becomes burning hot. The temperatures of the bright areas on Mars are down to freezing, that is to say, 32° F, while the dark areas have a temperature of 50 to 60° F which is not unlike conditions in New York, Philadelphia or Washington on a bright day in March and April. The radiometric measurements therefore support the conclusions drawn from the visual observations, and make the explanation quite plausible that the bright areas are so cool because they are high plateaus.

By setting the thermocouple receivers on the eastern and the western limbs or edges of Mars we found that the morning or sunrise side of the planet is much cooler than the afternoon or sunset edge of Mars. This is of course similar to conditions on this earth, the main difference being that on Mars the temperature falls to a much lower value at night, when the temperature drops down to — 80° F or perhaps even lower. This is owing to the fact that the atmosphere of Mars is rare, which facilitates cooling of the surface by radiation.

An interesting experience was the observation of the change in temperature of the surface with advance in the summer season on the southern hemisphere of Mars. We all know of the intense cold

in Alaska and Siberia, during the winter, followed by a short warm summer. Similarly, our measurements on the polar caps of Mars indicated temperatures down to — 90° F. in winter. But in the summer season, when the south polar cap had quite disappeared, the temperature was up to 50° F.

An interesting result of our radiometric measurements is the high temperatures observed on the dark areas on Mars. These temperatures are up to 50 to 60° F., which is equivalent to a warm spring or autumn day. As already stated, to the eye these dark areas have the appearance of vegetation. But this vegetation is probably quite unlike the plant life with which we are most familiar. It must be of a type that will withstand extremely dry weather and intense cold. Mars has but little water. Hence it is evident that what little vegetation may be present on Mars must be adapted to a dry climate.

Although the intensity of the sun's rays falling upon Mars is only about one half as great as falls upon the earth, the surface of Mars absorbs and utilizes about 85 per cent. as much of the total incident solar radiation as does the earth. Hence it seems evident that the temperature of the surface of Mars should rise almost as high as that of the earth.

The observed high local temperatures on Mars can be explained best by the presence of vegetation which grows in the form of tussocks or thick tufts, such as the pampas grasses, and the mosses and lichens which grow in the dry tundras of Siberia. The upper surface of such vegetation has a high absorption for sunlight, while the part beneath has a low heat conductivity. Hence from the very nature of the growth, namely, in tufts, but little heat is conducted to the ground, which may be frozen. For example, travelers in Siberia have found that the temperature of the top layers of the tundra mosses may be up to 75° F., while the ground only an inch or two underneath was frozen.

I think that the assumption of the presence of vegetation growing in tussocks is a reasonable explanation of the observed high temperatures on Mars; and it is in harmony with the visual observations which show changes in the dark areas with changes in the seasons. But the term high temperature is merely relative. With noonday temperatures of only 40 to 60° C. even on the hottest spots on the equator and with exceedingly low temperatures at night it seems evident that any vegetable or animal life that may exist on Mars must be adapted to withstand great extremes in temperature and humidity. From the way animals and plants adapt themselves to conditions on our deserts it seems possible for life to adapt itself to conditions on Mars.

Part of this adaptation would consist in being inactive over much longer periods than occur on this earth. Animal life would have to be troglodytic, able to burrow deep and hibernate or able to migrate or able to withstand the intense cold in a benumbed state as do, for example, the torpid grasshoppers, wasps and ants which one finds on warm days in winter. Life on Mars can not be very pleasant, especially in the equatorial region where it would be a continuous process of thawing out and limbering up in the forenoon and a reversal of the process in the afternoon. In the Martian polar regions, where the summer day is almost six months long, temperature variations would not be so extreme, and living matter, if present, would not be subjected to such short periodic changes in activity as occur on the equator. The cycle of reproduction and development of the living cell would not be subjected to such extreme temperature conditions. Similarly, the quiescent period during the prolonged winter would be free from interruptions.

Thus ends my story of temperature conditions on Mars. It was my good fortune to be the first to measure the temperature of Mars. "Sic volvere parcas"—thus spin the fates.

HOW MOUNTAINS ARE MADE

By Dr. WM. BOWIE

U. S. COAST AND GEODETIC SURVEY

THE earth is a solid globe, 8,000 miles in diameter. It has no liquid interior with volcanoes as chimneys. It is subjected to great strains as a result of the erosion of materials from the land and their piling up at the mouths of rivers. Earthquakes occur daily, though most of them can only be detected by the seismograph, one of the most sensitive instruments of the world. When an earthquake occurs, the land is moved only a few inches or feet, showing the rocks are after all not very strong. The earth is not going to blow up, nor will it collapse under our feet. But in the millions of years to come some parts of the land areas will be in the oceans, and some portions of the oceans will be added to continents or may be changed to islands. These things have occurred in the past and they will continue during the ages to come. There is no such thing as an everlasting hill.

Every continent has mountains, which not only add to the grandeur of the landscape but are of great importance to us in our daily lives. Irrigation projects in arid regions and hydroelectric

power plants are dependent upon the mountain streams and rivers. Without the mountains, which make the winds pay tribute in the form of rain, some parts of our area would indeed be deserts.

We should not consider mountain areas as great wastes, nor should we think of the great ocean areas as being of no importance except for fishing and the navigation of ships. If it were not for the oceans there would be no rain and without rain how would food be obtained?

The mountains have been mysteries from the time human beings began asking "why." Many explanations have been made as to their origin, but it is only in recent years, and as a result of researches principally by the United States Coast and Geodetic Survey, that we are able to advance a satisfactory explanation.

It has been found that the earth has a crust of rock resting on denser materials, which, though rigid to forces acting for short periods of time, will yield like putty or sealing wax when the forces act for tens, hundreds or thousands of years. The crust therefore floats, just as logs will float on a mud flat.

The reason we have continents and islands standing up above the oceans is because the crustal material under the ocean is denser than that under the land.

A chestnut log will stand out of the water higher than an oak log of the same size, because it is not so dense. For the same reason the lighter crust under continents stands up higher than the denser crust under the oceans.

Except for a thin layer of soil composed of sand, clay and loam the earth is made up of rock. The outer rock is mostly sandstone, limestone and shales, all of which were formed from materials washed down from the land and later compacted and cemented. Lower down are granite and basalt not formed from sediments.

Probably no rock seen at the surface came from a depth greater than twenty-five miles. No one knows what kinds of rock exist at lower depths. They may be ores, containing iron, lead or copper or even the precious metals. We know that the average density of the earth is about twice as great as that of the rocks at the surface. We do not know whether the greater density of the interior rocks is due to the presence of metals or to compression by the great weight of the outer rocks.

The mountain is a late comer on the earth. Rain has only been falling for about a billion and a half years, and before the rain fell there was nothing to disturb the face of the earth.

Each mountain system existing to-day is occupying a part of the earth's surface, which was earlier a part of an ocean or a great

inland sea. The distorted stratified rocks which can be seen in any mountain region are formed from sand and mud, carried by rivers and laid down in large bodies of water. Most of these rocks contain fossils of marine plants and animals. The mountains were formed along the margins of water into which the rivers and streams dumped the materials which were washed away by the rains. The sediments were accumulated in vast beds having thicknesses as great as six miles.

The weight of these sediments covering thousands of square miles, in rather thin strips along the margins of the oceans and seas, was tremendous. This load disturbed the crust floating placidly on the softer interior material. The crust beneath the sediments was forced down into hotter regions where it took up the greater temperature. This made the crust expand as a result of chemical or physical processes and a mountain system was formed.

The most delicate and accurate measurements by the U. S. Coast and Geodetic Survey and geodetic organizations of other countries show that the earth's crust everywhere is in equilibrium, just as an ice field on a quiet ocean is in equilibrium. Unless there is melting of the ice or there are winds and currents disturbing it there will be no ice ridges formed nor will any block be forced up or down.

Since the earth's crust is now in equilibrium, it must have been so in the past. Therefore when a large part of the crust changed the elevation of its surface from one to five miles there must have been a change in its density.

Before the rain first began to fall, about a billion and a half years ago, there were no mountains, but the earth's surface was not smooth like a ball. There were broad hollows and wide high areas. We know this as the result of researches by Dr. Henry Washington, of the Geophysical Laboratory of the Carnegie Institution of Washington. He discovered that the lava rocks found on the islands of the oceans have the heavier chemical elements present in larger percentages than the lava rocks found on the continents. Therefore, the rocks under the oceans are heavier than those under the continents, and the crust under the oceans is not so thick as under the land.

This condition existed when the rain began to fall. The water collected in the hollows, forming the oceans and seas, and the high land stood out as continents and islands.

Why the rocks in some parts of the crust should be heavier than in others no one knows. This is a problem some scientific investigators are working on, but it may never be solved.

We may infer that the earth's surface was too hot for water to remain on it before the sedimentary rocks began to be formed. Previously, the water must have existed as vapor or steam.

How long the earth has existed as a solid mass no one can guess, but it must have been several billions of years.

How long will it support life? We do not know. But if its surface temperature was 212° F., or the boiling point of water, one and a half billions of years ago, and is now about 50° as an average, it will be many millions of years before it will be too cold for life to exist. We need not worry about what takes place after that.

Some great force must have been at work to lift up great rocky masses like the Himalayan and Andes mountains, with peaks four or five miles above sea level. A single cubic mile of rock weighs twelve billion of tons, while a mountain system has thousands of cubic miles of rock which were once below sea level.

The earthquakes, occurring daily with only slight changes in elevation of the surface of the earth, show that great forces do not accumulate to shove up a mountain as a break in the crust all at once. If this should occur the earth would be shaken so hard that great waves from the oceans would roll right over the continents and drown all land animals. This would have occurred with the formation of each mountain system. A century ago scientists believed this was the way in which mountains were formed, but no one does now.

After considering all the geologic and geodetic data we are forced to the conclusion that the shifting of material over the earth's surface by rain water running from the land to the oceans and seas, carrying great masses of sand and mud and depositing them along the coasts, is the real cause of mountains. Each little muddy stream is doing its bit, and each big river, like the Mississippi, the Amazon and the Nile, is doing its big bit, to make new mountains which we expect to appear in a future geological age—possibly several hundred millions of years from now.

While new mountains will be formed where great beds of sediments are accumulating, the existing mountains will be worn down. The United States is losing six hundred cubic miles of material each ten thousand years. The mountains are gradually disappearing. Under the mountains the crust is being buoyed up with the wearing away of its surface. The crustal materials are coming into cooler regions. The result of this is that after the mountains disappear the crust below will contract from the loss of heat. Where we now have mountains there will be low ground or there may be seas or parts of the oceans into which new sediments will be dumped.

Some areas now occupied by mountains have been above and below sea level several times.

There has been plenty of rain to cause all the mountains of the earth. The average rainfall now is thirty inches each year. This means a mile of rain in two thousand years. If the rainfall has been the same during the sedimentary age of one and one half billions of years nearly a million miles of rain has fallen.

Of course, the same water has been used over and over again, for if all the water were spread out evenly over the earth its depth would be slightly less than two miles.

From all the available evidence we must conclude that rain, due to repeated evaporation and precipitation, has caused the wearing away of the soil and the laying down of great masses of sediments along the margins of oceans and seas and has produced the great changes in the elevation of the earth's surface.

Earthquakes and volcanoes and the formation of minerals, oil and coal are merely incidents in the general process. We have earthquakes as the sediments depress the crust, as the crust below them swells up to form mountains, as the crust is buoyed up under the areas undergoing rapid wearing away, and as the crust cools and sinks under worn-down mountain areas.

Volcanoes and lava outflows are probably caused by the cracking or rifting of the crust in areas where mountains and islands are in the process of formation. The cracks must extend deep enough to reach rock that is very hot but solid under the great load of the outer rock. With the cracking of the crust the deep rock becomes soft and flows to the surface.

Don't be afraid of old mother earth. As I said before she is not going to collapse nor is she going to blow up. She will adjust herself to strains once in a while, and the earthquake will tell you when she does. If we should keep away from the ocean shores and should live in tents she would not hurt us. Few would get hurt if we knew where earthquakes are likely to occur and if we guarded against them by erecting earthquake-proof buildings. We shall build better in the future after we have made earthquake surveys. It is expected such surveys will be commenced in this country in the near future.

THE PHYSIOLOGICAL BASIS OF ATHLETIC RECORDS[1]

By Professor A. V. HILL, F.R.S.
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In the study of the physiology of muscular exercise there is a vast store of accurate information, hitherto almost unexploited, in the records of athletic sports and racing. The greatest efforts and the most intense care have been expended in making what are really experiments upon these subjects, and the results obtained represent what may justly be described as a collection of natural constants of muscular effort in the human race. It is the purpose of this address to discuss certain aspects of the data available in connection with various forms of racing, and to see how far physiological principles at present known underlie them.

Sources of Information

The most complete set of records available, for a great variety of sports, is to be found in "The World's Almanac and Book of Facts," published by the New York *World*. Much of the information here presented was obtained from the 1925 edition of that work; similar but less extensive data can be found in our own Whitaker's Almanack. In addition, various books on horse-racing, on swimming and on rowing have been searched for suitable material. The study of such data is not new. In most cases, however, it has been carried out not from the physiological but purely from the statistical standpoint; insufficient knowledge of the underlying physiological principles was available to make it profitable to ask for the why and wherefore. Recent developments, however, of the scientific study of muscular effort in man have indicated certain broad lines on which some at any rate of the relations so established can be explained. I will not deal further with the statistical analysis of the facts, beyond referring to an extremely interesting and suggestive collection of them given in a paper by A. E. Kennelly, entitled "An approximate law of fatigue in the speed of racing animals," published in the *Proceedings of the American Academy of Arts and Sciences,* vol. xlii., p. 275, 1906. Some, indeed, of my data are taken directly from that paper.

[1] Address of the president of the Section of Physiology of the British Association for the Advancement of Science, Southampton, 1925.

Fatigue as the Determining Factor

An important and interesting problem for any young athlete is presented by the question, "How fast can I run some given distance?" The maximum speed at which a given distance can be covered is known to vary largely with the distance. What are the factors determining the variation of speed with distance? How far, knowing a man's best times at two distances, can one interpolate between them for an intermediate distance, or extrapolate for a distance greater or less? Obviously the answer to such questions depends upon the factor which in general terms we designate fatigue. Fatigue, however, is a very indefinite and inexact expression; it is necessary to define it quantitatively before we can employ it in a quantitative discussion such as this. There are many varieties of fatigue, but of these only a few concern us now. There is that which results in a short time from extremely violent effort: this type is fairly well understood; there is the fatigue, which may be called exhaustion, which overcomes the body when an effort of more moderate intensity is continued for a long time. Both of these may be defined as muscular. Then there is the kind which we may describe as due to wear-and-tear of the body as a whole, to blisters, soreness, stiffness, nervous exhaustion, metabolic changes and disturbances, sleeplessness, and similar factors, which may affect an individual long before his muscular system has given out. Of these three forms of fatigue the first one only is as yet susceptible of exact measurement and description. The second type may quite possibly come within the range of experiment at no distant date. The third type is still so indefinite and complex that one can not hope at present to define it accurately and to measure it. Undoubtedly, however, all these types of what we call "fatigue" influence—indeed, determine—the results which are to be presented.

Presentation of Data

The data will be exposed throughout this discussion in graphical form, and in every case but one (Fig. 5) the quantities plotted are the speed as ordinate and the time, or some function of the time, as abscissa. The reason for taking the *time* occupied in a race as one of our variables is simple; the problem before us, physiologically speaking, is, clearly, *how long can a given effort be maintained?* The length of time is given by the abscissa as the independent variable; the magnitude of the effort, or some function of it, as represented by the speed (that is, by the average speed over the race considered), is given as ordinate. It will be shown below, as Kennelly indicated in his paper, that the ideal way to run a race,

possibly not from the point of view of winning it, but certainly from that of breaking the record for the distance, is to run it at constant speed. In those performances which have attained to the dignity of a world's record it is unlikely that this criterion has been to any very large degree neglected. Apart, therefore, from the fact that there is no speed of which we have any record except the average speed, we are probably not far wrong in using the average speed as a fairly exact measure, or at any rate as a function of the effort involved.

In one case only (Fig. 6) the time occupied in the race has been given on a logarithmic scale: no great virtue attaches to the logarithm, but if 75 yards and 100 miles are to be shown on the same diagram in a readable form it is necessary somehow to condense the abscissae at the longer times. As a matter of fact, from the standpoint of an athlete, one second in ten has the same importance as ten seconds in a hundred, as a hundred seconds in a thousand; in this sense, therefore, a logarithmic scale of time most truly represents the duration of an effort. Such a scale, however, has not been used for any ulterior reason, but only, as in Fig. 6, to get all the available data on to one diagram.

Running and Swimming: Shorter Times

In Fig. 1 all the important world's records are presented, average speed against time, for men and women running and for men

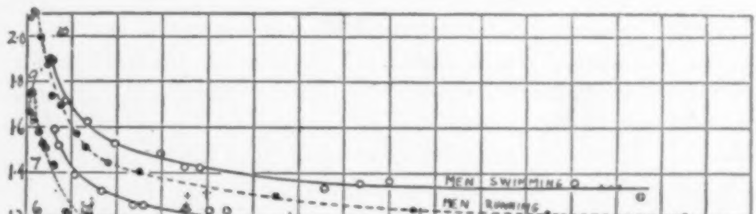


FIG. 1.—World's records for men and women swimming and running: average speed in yards per second against time in seconds. *Note.*—The scale for swimming is five times as great as for running. The observations for men rowing an eight-oar boat are on the same scale as running and are referred to later in the text.

and women swimming. The crosses representing men rowing in an 8-oar boat will be discussed later. It is obvious in all four cases that we are dealing with the same phenomena, a very high speed maintainable for short times, a speed rapidly decreasing as the time is increased and attaining practically a constant value after about 12 minutes. There are no reliable records, in the case of swimming, for times of less than about 50 seconds, so that the curves can not be continued back as far as those for running. There can, however, be no doubt that the curves for running and swimming are essentially similar to one another and must depend upon the same factors. In running, starting inertia is the cause of the initial upward trend of the curves: a maximum average velocity is attained in the case of men for about 200 yards, of women for about 100 yards; after that a rapid decrease sets in, ending only when the time has become 10 or 15 minutes, the distance two to three miles. The phenomena shown in Fig. 1 are susceptible of a fairly exact discussion.

Oxygen Intake, Oxygen Requirement and Oxygen Debt

In recent papers my colleagues and I have tried to emphasize the importance of a clear distinction between the oxygen intake and the oxygen requirement of any given type and speed of muscular effort. When exercise commences, the oxygen intake rises from a low value characteristic of rest to a high value characteristic of the effort undertaken. This rise occupies a period of about two minutes; it is nearly complete in 90 seconds. The oxygen used by the body is a measure of the amount of energy expended: one liter of oxygen consumed means about five calories of energy liberated, enough to warm 5 liters of water one degree Centigrade—expressed in mechanical energy, enough to raise about one ton seven feet into the air. It has been established, however, that the oxygen need not necessarily be used during the exertion itself. The muscles have a mechanism, depending upon the formation of lactic acid in them, by which a large amount of the oxidation may be put off to a time after the exercise has ended. The recovery process, so called, is a sign of this delayed oxidation: it is just as important to the muscle as recharging to an electric accumulator. The degree, however, to which the body is able to run into debt for oxygen, to carry on not on present but on future supplies, is limited. When an oxygen debt of about 15 liters has been incurred the body becomes incapable of further effort: it is completely fatigued. In anything but the shortest races our record-breaking athlete should finish with something near a maximum oxygen debt, otherwise he has not employed all his available power, he has not done himself full justice. The maximum effort, therefore, which he can exert over a given interval

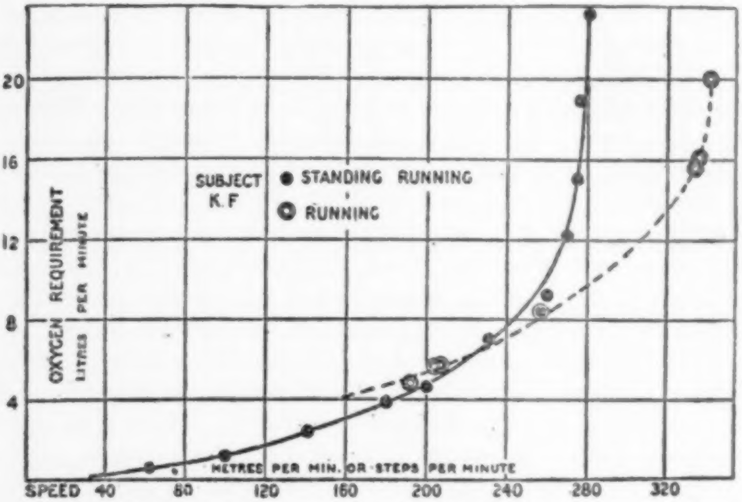


FIG. 2.—Observations of oxygen requirement of K.F. running and standing-running at various speeds. Horizontally, speed: running, meters per minute; standing-running, steps per minute. Vertically, oxygen requirement per minute, liters.

depends upon the amount of energy available for him, upon (*a*) his maximum oxygen intake (that is, his income) and (*b*) his maximum oxygen debt (that is, the degree to which he is able to overdraw his account). These maxima are fairly well established for the case of athletic men of average size—about 4 liters per minute for the one, about 15 liters for the other.

It is possible for a man to make an effort far in excess of any contemporary supply of oxygen. This effort will require oxygen afterwards, and the total oxygen needed per minute to maintain the exercise can be measured. It is what we call the "oxygen requirement" characteristic of the effort involved. Now experiments have shown (see Fig. 2) that the oxygen requirement varies very largely with the speed: it increases far more rapidly than the speed, more like the second or third power of the speed, so that high speeds and intense efforts are very wasteful. These facts enable us approximately to deduce the general form of Fig. 1.

Imagine an athlete with a maximum oxygen intake of 4 liters per minute,[2] capable of running until his maximum oxygen debt has been incurred of 15 liters. If he runs for 15 minutes the total oxygen available during the exercise and in arrears is $15 \times 4 + 15 = 75$ liters: an effort can be made requiring 5 liters of oxygen per

[2] Assumed, for the sake of simplicity in calculation, to commence as soon as the race begins. For a more accurate calculation the gradual rise of the oxygen intake at the beginning of exercise can be taken into account.

minute. Imagine, however, that he exhausts himself not in 15 but in 5 minutes: the total oxygen available during or in arrears is $5 \times 4 + 15 = 35$ liters. He may exert himself more violently, therefore, with an effort equivalent now to 7 liters per minute. Imagine next that he runs himself to exhaustion in 2 minutes: $4 \times 2 + 15$, *i.e.*, 23 liters of oxygen, are available, 11.5 per minute; a correspondingly greater effort can be made. By such calculations it is possible from Fig. 1 to deduce a relation between oxygen requirement and speed. Taking the case of a man swimming, the result is shown in Fig. 3 on the assumption of a maximum oxygen debt of 15 liters, a maximum oxygen intake of 3.5 liters per minute, and the supposition that at the end of the race the performer is completely exhausted. A similar calculated curve is given for the case of running, on the hypothesis of a maximum oxygen debt of 15 liters and a maximum oxygen intake of 4 liters per minute. These curves are similar in character to those shown in Fig. 2 for the cases of running and standing-running, which have been investigated in the laboratory. There can be little doubt that the factors here described are the chief agents in determining the form of the curves given in Fig. 1.

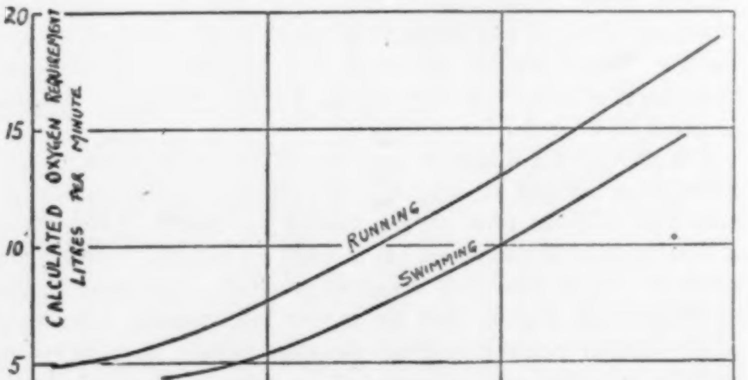


FIG. 3.—Oxygen requirement, running and swimming, of record-breaking athletes, calculated from curves of Fig. 1, on the assumption that at the end of a race the performer is completely exhausted, having attained his maximum oxygen debt. Maximum oxygen debt assumed = 15 liters for both. Maximum oxygen intake assumed: for running = 4 liters per minute; swimming = 3.5 liters per minute. Method of calculation described in the text.

Limits of the Argument

It is obvious that we must not pursue the argument too far. A man can not exhaust himself completely in a 100 or a 200 yards race: even 300 yards is not sufficient to cause an extreme degree of exhaustion, though a quarter-mile, in the case of a first-class sprinter, is enough, or almost enough, to produce complete inability to make any immediate further effort. We have found an oxygen debt of 10 liters even after a quarter-mile in 55 seconds. It is obvious, therefore, that we can not pursue our argument below times of about 50 seconds, that the maximum speed is limited by quite other factors than the amount of energy available. It is not possible in any way to release energy explosively for very short intervals of effort: other factors determine the maximum speed, factors mechanical and nervous. Neither can the argument be applied to very long races, where—as we shall see below—other types of exhaustion set in.

Comparison of Men and Women; Swimming and Running

There are certain characteristics of these curves which are of interest. In the first place those for men and women are almost precisely similar. For a given time of swimming the maximum speed for a woman appears throughout the curves to be almost exactly 84 to 85 per cent. of that for a man. The curve relating oxygen requirement to speed, in the case of swimming, is not known from experiment, nor are the maximum oxygen debts and the maximum oxygen intakes known for women with any certainty. It would be very interesting to determine them, were volunteers forthcoming. If we asume what is roughly true, that the energy expenditure rises approximately as the square of the speed, we may conclude that a woman swimming is able to exert, per kilogram of body weight, about 72 per cent. of the power expended by a man. Women are well adapted to swimming: their skill in swimming is presumably just as great as that of men; the difference in the maximum speed for any given time can be a matter only of the amount of power available.

In running, the same type of comparison may be made, though here not over the same range of times. For anything but the shortest races the maximum speed of a woman is almost precisely 79 per cent. of that of a man running for the same time. For very short times, 5 to 10 seconds, the ratio is greater, namely, 84 per cent. Here again there would seem little reason to attribute the difference of speed, at any rate for the longer races, to anything but a difference in the maximum amount of power expendible

over the period in question. Assuming again, as an approximate means of calculation, that the energy used per minute varies as the square of the speed, we see that a woman running is able to liberate in a given time only about 62 per cent. of the energy expendible by a man of the same weight. It is probable that this ratio between men and women, as determined by swimming and by running, respectively, is really the same in either case, and that the apparent difference depends upon an inexactness in the simple laws we have assumed for the variation of energy expended with speed. It would seem fair to take the mean of these two values, 67 per cent.—that is, about two thirds—as the ratio of the amount of energy expendible by a woman in a given time as compared with that by a man of the same weight. It would be of great interest—and quite simple—to test this deduction by direct experiment on women athletes.

Men and Women Jumping

A further interesting comparison between men and women may be found in the records of high jumps and long jumps. The world's record long jump for a man is 25.5 ft., for a woman 16.9 ft. The high jump records are respectively 6.61 ft. and 5 ft. At first sight, when compared with running, these records for women seem extraordinarily poor: the high jump is only 75.5 per cent., the long jump only 66 per cent., of that for men. Such a conclusion, however, rests upon a misunderstanding, almost like that which makes many people believe that if a man could jump as well as a flea he could easily clear the top of St. Paul's Cathedral. It is a matter only of elementary mechanics to show, on the assumption that a woman can project herself vertically with a velocity proportional to that with which she can project herself horizontally, the constant of the proportion being the same as for the case of a man, that both the high jump and the long jump in the two sexes should be in the ratio *not of the velocities but of the squares of the velocities.* The maximum range and the maximum height of a projectile vary as the square of the velocity of projection. Thus it is right to compare, for men and women, not the height of the high jump or the distance of the long jump, but the square roots of these quantities, if we wish to study their relative performance in jumping as compared with running. This being so, we find that the high jump of a woman, as measured by its square root, is 87 per cent. of that of a man[3]; the long jump, measured in a similar

[3] It would really be fairer to compare the heights jumped, less the initial heights of the centers of gravity, say 3.1 feet and 2.8 feet, respectively. This gives 2.2/3.51 = .63 as the ratio of the heights, of which the square root is .79, a close agreement with the long jump.

way, is 81.5 per cent. These compare closely with their relative performances for very short times of running, where a woman, as shown above, can run 84 per cent. as fast as a man. It is amusing to find simple mechanics explaining such apparent differences between the sexes.

The Characteristic Oxygen-Requirement-Speed Curve

The curves given in Fig. 2 define the economy with which movements are carried out. By such means can be shown the amount of energy required, in terms of oxygen used, in order, say, to run or swim for a minute at any given speed. The curves will vary largely from one individual to another. Some men move more efficiently than others at all speeds: A may be more efficient at one speed than B is, but less efficient at another. For most kinds of muscular exercise the characteristic curve of Fig. 2 is ascertainable by experiment. In some cases, as in swimming, experimental difficulties might be considerable, at any rate at higher speeds. It is obvious, however, that such a curve must exist for any person performing any kind of continuous muscular exercise. In it we have a characteristic of that given individual for that particular form of work.

Skill

Some people are much more skilled than others. To a large degree, of course, the skill and grace associated with athletic prowess is natural and inborn; to a large degree, however, it can be produced by training and breeding. All the movements required in the violent forms of muscular exertion here discussed are rapid ones, far too rapid to be directly and continuously subject to the conscious intelligence: they are largely, indeed mainly, reflex, set going by the will but maintained by the interplay of proprioceptive nervous system and motor apparatus. The nature of muscular skill can not be discussed here; possibly, however, above all other factors it is the foundation of athletic prowess. Such skill has a physiological basis as it has a psychological aspect. It is a fit subject for discussion alike by physiologists, psychologists, students of physical training, athletes, masters and workmen. The further study of skill is likely to be most fruitful in many branches of human endeavor. Here I would only remark that the forms of the characteristic curves of Fig. 2 depend upon the skill of the subject in ordering his movements, just as the "miles per gallon" of a motor-car depends upon the skill of those who designed and adjusted its timing gear and its magneto. Given incorrect adjustment due to lack of skill, given imperfect timing of

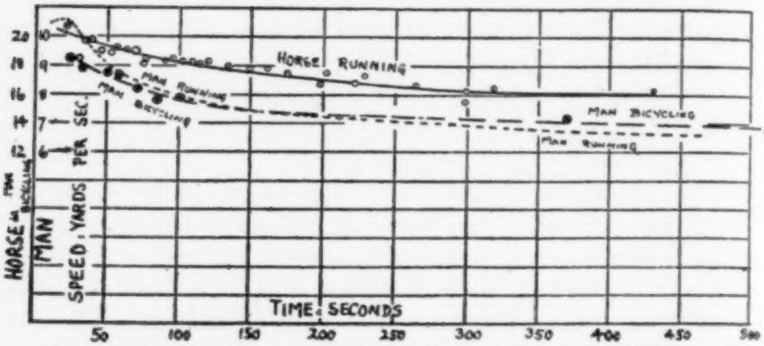


FIG. 4.—Records for horse running and man bicycling; dotted curve for comparison, man running, taken from Fig. 1. Horizontally, time in seconds; vertically, average speed yards per second. *Note.*—The horse, and the man bicycling are shown on half the scale of the man running. The records for bicycling are the unpaced professional records against time. The records for horses were made in America.

the several parts of the mechanism, given unnecessary movement and vibration, the whole system will be inefficient. Fundamentally the teaching of athletics for anything but the shortest distances consists in training the performer to lower the level of his characteristic curve, to carry out the same movements at a given speed for a smaller expenditure of energy.

Bicycling and Horse-Running

Not all forms of muscular exertion are so violent, involve so great an expenditure of energy, when carried out at the highest speed, as running and swimming. In Fig. 4 are two examples of this fact, horse-running and bicycling. For horse-running a long succession of records on American horses are plotted on the topmost curve: below are the records of men bicycling, the unpaced professional records, made not in a race but against time. Most bicycle races are useless for our purpose: the competitors proceed in groups, trying one to ride behind the other to avoid wind resistance, and the speed may be absurdly low. Paced records are of little value because the efficiency of the wind-screen provided by the pacing apparatus is not standardized. These professional records, however, made unpaced, simply with the intention of breaking the record, are probably reliable, and they form a reasonably smooth curve. Plotted on the same diagram for comparison is a curve to represent a man running, a replica of that of Fig. 1. The first two curves are on twice the scale of the third, since a running horse and a bicycling man can go about twice the speed of a run-

ning man. It is obvious at once that neither of these two curves falls anything like so rapidly as does that of a running man; fatigue does not so soon set in: the amount of energy expended at the highest speed must be much less than in a running man. This conclusion, indeed, is obvious to any one who has tried to ride a bicycle fast. It is impossible to exhaust oneself rapidly on a bicycle: the movements are too slow, they involve too little of the musculature of the body; it would require some minutes to produce by bicycling a state of exhaustion easily attainable within a minute by running. The curve for horse-running is almost parallel to that for bicycling; presumably, therefore, the movements of a horse are so arranged that the extreme violence of effort possible in a human "sprinter" is unattainable: possibly the movements are too infrequent, or the qualities of the horse's muscles are so different, that the kind of fatigue rapidly attainable in man is not possible in the horse; possibly the horse will not "run himself out" so completely as a man.

Bicycle Ergometers

The curves of Fig. 4 are of interest in connection with the numberless experiments which have been made with bicycle ergometers. Nearly all the laboratory observations on man, in connection with muscular exercise, have been made with that implement. It has been obvious to my colleagues and myself during the last few years that the types of exercise chiefly adopted by us, running and standing-running, are more exhausting and require a far greater expenditure of energy than those employing the bicycle ergometer. In rowing and in pedalling a bicycle it may not be possible to attain respiratory quotients of 2 or more during or shortly after exercise. After running, or standing-running, however, very high values are attained, due to the fact that these latter forms of exercise, at the highest speeds, are so very much more energetic than the slower movements of rowing or bicycling. It is speed and frequency of movement which determine the degree of exhaustion produced by it. To exert a powerful force in a moderate rhythm is not anything like so tiring as to exert a much smaller force in a frequent rhythm: hence the reason for "gearing up," as in the bicycle and in the long oars of a rowing-boat.

Horse-racing

The fact that running is not so exhausting to a horse as to a man is well shown by the records of Fig. 5. There the small circles represent the best English records of horse-racing between the

years 1721 and 1832. Speed in meters per second is given against kilometers distance. The larger circles represent the best of some more recent English records, from 1880 to 1905. D, O and L represent respectively the Derby, the Oaks and the St. Leger. It will be seen how little the speed falls off for the longer races: six or seven kilometers are run at the same speed as one or two. There is, indeed, a visible tendency for the curve to rise towards the left, as in Fig. 4; there is, however, no obvious further fall of the curve towards the right after about two kilometers. Such a statement would seem preposterous to a human runner if applied to himself. Either the horse can not exhaust himself so rapidly as a man, or he can not be induced by his rider to go as hard as he ought. A man may be able to force himself to a greater degree of exhaustion than his rider can force a horse. An amusing incidental point brought out by Fig. 5 is the fact that the small circles and the large ones are intermingled. The horses of 150 years ago could run just as fast as their modern successors—a fair comment on the doctrine that the improvement of the breed of horses is the chief and a sufficient reason for encouraging the continuance of horse-racing—even in time of war.

The Logarithmic Graph

Let us pass now to a consideration of the last diagram, Fig. 6. There average speed in a race is plotted against the logarithm of the time occupied in it, the logarithm being employed, as stated

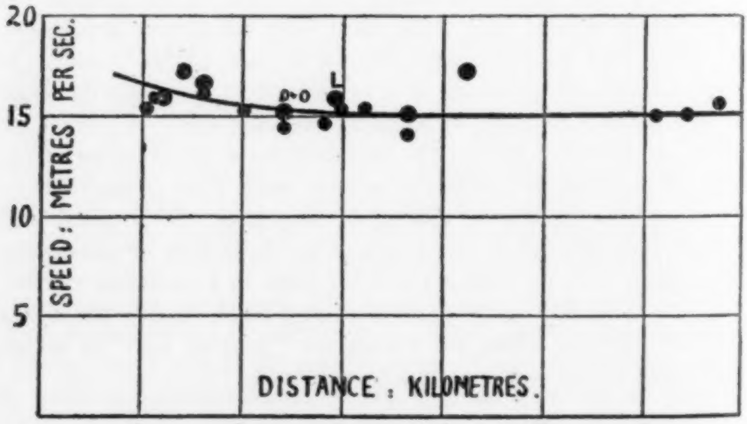


Fig. 5.—Records for horse-races. Small circles = old English records, 1721–1832. Large circles = later English records, 1880–1905. D = Derby, O = Oaks, L = St. Leger. Average speed, meters per second, against distance in kilometers.

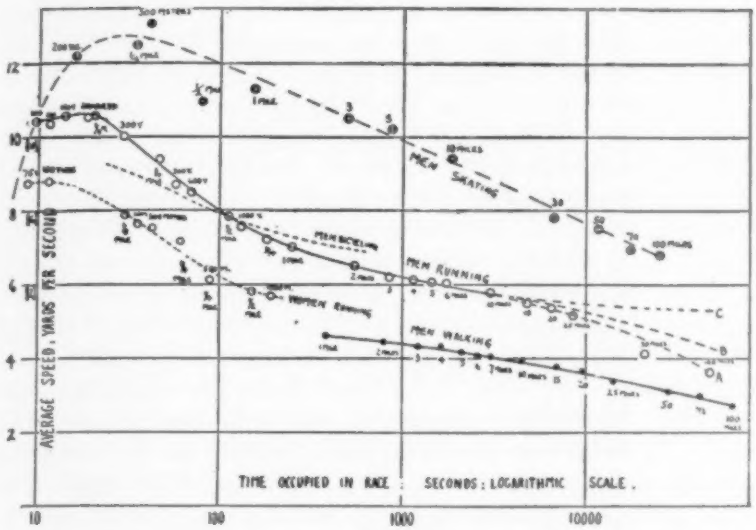


FIG. 6.—Records for men skating, bicycling, running and walking, and for women running. Horizontally, logarithm of time occupied in race; vertically, average speed in yards per second. The same scale is used throughout, except for bicycling, where half the scale is employed, as shown in square brackets. The curve for men running appears to be somewhat doubtful beyond 10 or 15 miles, and three alternative curves are shown by broken lines.

above, for the purpose of including all records from 75 yards to 100 miles in the same picture. That people think, to some degree, in logarithms, although unconsciously, is shown by the fact that the records which men have thought it worth while to make are distributed approximately uniformly over the picture from left to right. Fig. 6 presents the data of athletics perhaps more clearly than any other. The initial rise of the curve for men running, which is due to starting inertia, is very obvious. The rapid fall beyond 220 yards is clearly seen. It is obvious that the 100 and the 220 yards (1/8 mile) records are better than those lying in their neighborhood, that the quarter-mile record is extremely good, the 500 yards record very bad, by comparison with its neighbors. This diagram should enable any enterprising and scientific athlete to select the records most easy to break: let him try those for 120 yards, for 500 yards, for three-quarter-mile, for three miles, but not for 220 yards, quarter-mile, one mile and six miles.

LONG-DISTANCE RECORDS

In Fig. 1 we saw that the speed fell to what seemed to be practically a constant level towards the right of the diagram: this fall

represents the initial factor in fatigue. On the logarithmic scale, however, where the longer times are compressed together, the curve continues to fall throughout its length. This later fall is due to factors quite different from those discussed above. Consideration merely of oxygen intake and oxygen debt will not suffice to explain the continued fall of the curve. Actually the curve beyond 10 miles seems to some degree doubtful. Apparently the same extent of effort has not been lavished on the longer records: the greatest athletes have confined themselves to distances not greater than 10 miles. The curve A drawn through all the points has a suspicious downward bend in it, which suggests that if Alfred Shrubb or Nurmi had tried to break the longer records they would have done so very effectively. Possibly the true curve lies more like the continuation C: possibly it may be intermediate as shown at B. It would seem doubtful, indeed, whether the running curve and the walking curve are really to meet at about 150 miles. The most probable continuation of the running curve would seem to be somewhere between the lines B and C.

The continued fall in the curve, as the effort is prolonged, is probably due to the second and third types of fatigue which we discussed above, either to the exhaustion of the material of the muscle, or to the incidental disturbances which may make a man stop before his muscular system has reached its limit. A man of average size running in a race must expend about 300 gms. of glycogen per hour; perhaps a half of this may be replaced by its equivalent of fat. After a very few hours, therefore, the whole glycogen supply of his body will be exhausted. The body, however, does not readily use fat alone as a source of energy: disturbances may arise in the metabolism; it will be necessary to feed a man with carbohydrate as the effort continues. Such feeding will be followed by digestion; disturbances of digestion may occur —other reactions may ensue. For very long distances the case is far more complex than for the shorter ones, and although, no doubt, the physiological principles can be ascertained, we do not know enough about them yet to be able further to analyze the curves.

Women's Running Records

The women's curve, as far as it goes, is very similar to the men's. Some records again are better than others. An enterprising woman athlete who wants to break a record should avoid the 300 meters; she would be well advised to try the 500 meters.

It would be very interesting to have an intermediate point between 100 and 220 yards.

Bicycling and Walking

As before, the curve for men bicycling, which is drawn on twice the scale vertically of the running curves, is far less steep than they are. The conclusion from this was emphasized above. The walking curve is interesting—it is approximately straight. Physiologically speaking, there is not much interest in the shortest walking races, since here walking is artificial and extremely laborious; running at a considerably higher speed is much more easy. For longer distances, however, say from 10 miles onwards, we have probably in walking the most reliable data available for long-continued muscular effort. If we wish to study the exhaustion produced by exercise of long duration, walking-men may well provide the best subjects for our experiments.

Skating

There remains the top curve of all, that for men skating. The initial rise of the curve, due to starting inertia, is very obvious. The fall of the curve beyond the maximum is nowhere near so rapid as for the case of running. Clearly in skating a man is not able to exert himself with the degree of ardor that is possible in the more primitive exercise of running. Skill and restraint are necessary, as they are in bicycling: there are limits to the output. Moreover, the effort can be continued for a long time, at comparatively high speeds. It is interesting to note that a man can skate 100 miles at almost the same speed as another man can run one mile. The curve falls uniformly throughout as does the walking curve. Clearly the phenomena of gradual exhaustion could be well investigated in the case of skating. Here again it is obvious which records the aspiring athlete should attempt to break.

Rowing

There are only a few records available, and those lying between rather narrow limits, for the case of rowing. Taking the case of an eight-oar boat, I have been able to obtain very few reliable data. Kennelly gives records of crews rowing, for times from 305 to 1,210 seconds. Yandell Henderson, in the *American Journal of Physiology,* vol. lxxii., p. 264, 1925, gives five observations made upon the Yale crew of 1924. In addition there are records for the Henley course: these, however, are usually contaminated by the

speed of the water. The most reliable of the data have been plotted in Fig. 1 on the same scale as the running, on five times the scale of the swimming. The observed points, shown by crosses, are somewhat scattered. As far as they go, a mean curve through them would lie practically along the curve for women swimming, but of course on five times the scale. The interesting part of the curve to the left is lacking; it is obviously impossible to make observations on an eight-oar boat for periods of 20 seconds, starting inertia is too great and no result of any value could be obtained. It would, however, be of interest to obtain data as far back as possible; certainly the records of crews rowing in still water for a minute and above should be ascertainable, and they would help to fit rowing into the scheme outlined by the other types of muscular effort.

Work and Stroke Frequency in Rowing

In rowing the movements are slow: in an eight-oar boat, from 30 to 40 strokes per minute. According to observations by Lupton and myself the maximum efficiency of human muscular movement is obtained at speeds of about one maximal movement per second. In rowing, experience and tradition alike suggest that such a speed is about the optimum. In an eight-oar boat the recovery takes almost as long as the stroke, both occupying about one second. It is of interest how practical experience has gradually evolved a speed of movement which is almost exactly what a physiologist might have predicted as the most efficient. At a stroke of about 32 per minute the mechanical efficiency is apparently near its maximum. An enormous amount of work has to be done in propelling a boat at speeds like 10 to 12 miles per hour. According to Henderson, each member of the crew of an eight-oar boat must exert about 0.6 of a horse-power. Clearly if this enormous amount of external work is to be done it must be accomplished by working under efficient conditions: those conditions necessitate a stroke of a particular frequency; only when the race is very short is it permissible, in order to obtain a greater output, to work less efficiently by adopting a more rapid stroke. The stroke may rise to 40 per minute for a short distance: in such an effort the oxygen debt is accumulating rapidly and exhaustion will soon set in. The amount of work, moreover, will not be proportionately greater, probably only slightly greater, than at the lower frequency. The conditions which determine the speed of movement, the "viscous-elastic" properties of muscle, are what ultimately decide the length of the oars and the speed of movement in a racing-boat. It is interesting

to find—as, of course, was really obvious—how closely athletics is mixed with physiology.

Wastefulness of High Speeds

This last discussion leads us to the question of what determines the great wastefulness of the higher speeds. Why, returning to Fig. 2, does a speed of 280 steps per minute require 24 liters of oxygen per minute, while a speed of 240 steps per minute requires only eight liters of oxygen? The answer depends upon the variation of external work with speed of muscular movement. In a series of recent papers it has been shown that in a maximal muscular movement the external work decreases in a linear manner as the speed of shortening increases. At sufficiently high speeds of shortening no external work at all can be performed. In most of these athletic exercises, apart from the case of rowing, a large proportion of the mechanical work is used in overcoming the viscous resistance of the muscles themselves. At high speeds of running only a small fraction of the mechanical energy of the muscles is available to propel the body, once the initial inertia has been overcome. The speed of shortening is so rapid that little external work can be done. The work is absorbed by internal friction, or by those molecular changes which, when the muscle is shortening rapidly, cause its tension to fall off. When working against an external resistance, as in rowing, there is an optimum speed. If an effort is to be long continued it must be made at a speed not far from the optimum. When, however, the whole of the resistance to movement is internal, as in running, there is no optimum speed: the expense of the movement increases continually as the speed goes up; the faster we move, the greater relatively the price: our footsteps are dogged by the viscous-elastic properties of muscle, which prevent us from moving too fast, which save us from breaking ourselves while we are attempting to break a record.

Uniform Speed is the Optimum

The amount of energy required per minute to run or to swim, or, indeed, to propel oneself in any way, increases more rapidly than the speed—in the cases which have been investigated, approximately as the square of the speed. This mathematical relation is not exact: the facts can only really be described by a curve such as that of Fig. 2, but it simplifies the argument. From the form of the curve of Fig. 2, or from the variation of energy output as the square of the speed, we can immediately deduce that the most efficient way in which to run a race is that of a uniform speed

throughout. Imagine that a man runs a mile race in 4 minutes 30 seconds at a uniform speed of 6.52 yards per second: his energy expenditure is proportional to $4\frac{1}{2}$ times 6.52 squared; that is, 191.3 expressed in some arbitrary units. Imagine now that he runs it at two speeds, 6 and 7 yards per second, 780 yards at the lower, 980 at the upper speed: the total time is the same; the energy expended, however, is slightly greater, 192.3 instead of 191.3. This small variation of speed in the race has produced no serious increase in the energy expenditure. Let us imagine, however, that one portion of the race, 665 yards, is run at 5 yards per second, while another portion, 1,096 yards, is run at 8 yards per second. The total time occupied in the race is still 4 minutes 30 seconds. The energy expended, however, is greater, namely, 201.5 units. Even this, however, is not a very large increase; by running about half the time at 8 yards and half the time at 5 yards per second, the energy expended has been increased only about 5 per cent. as compared with that required for running at a uniform speed of 6.5 yards per second throughout. Although, therefore, theoretically speaking, the optimum fashion in which to run a race is that of uniform velocity throughout, comparatively large variations on either side of this velocity do not appreciably increase the amount of energy expended.

Possible Advantages of a Fast Start

There may, indeed, be advantages in starting rather faster than the average speed which it is intended to maintain. The sooner the respiration and circulation are driven up to their maximum values, the greater will be the amount of oxygen taken in by the body, the greater the amount expendible during the race. It is a common practice in mile races to start very fast and to settle down later to the uniform speed: this may have a physiological basis in the quickening up of circulation and respiration achieved thereby.

The Simple Mechanics of High-Jumping

One final point may be worthy of mention—this time connected with high-jumping and long-jumping. Recently I made a series of observations, with a stop-watch reading to 0.02 seconds, of the times occupied by a number of high-jumpers from the moment they left the ground to the moment they reached the ground again. With men jumping about five feet the time average about 0.80 second. Calculating from the formula

$$S = \tfrac{1}{2}gt^2,$$

where t is half the total time of flight, the distance through which the center of gravity of the body was raised must have been about 2.5 feet. The men competing must have had an original height of their center of gravity of about 2.7 feet. Thus, in the high-jump, their centers of gravity went about 5.2 feet high into the air. They cleared a height of five feet: they just managed to wriggle their centers over the bar. Now, paradoxical as it may seem, it is possible for an object to pass over a bar while its center of gravity passes beneath; every particle in the object may go over the bar and yet the whole time its center of gravity may be below. A rope running over a pulley and falling the other side is an obvious example. It is conceivable that by suitable contortions the more accomplished high-jumpers may clear the bar without getting their centers of gravity above or appreciably above it. Let us calculate, however, on the assumption that the center of gravity of a jumper just clears the bar. The world's record high-jump is 6.61 feet, the center of gravity of the performer being presumably about 3 feet high at rest. He raises it therefore 3.61 feet into the air, from which we may calculate that the whole time occupied in the jump is about 0.96 second. Seeing the amazing complexity of and the skill involved in the rapid movements and adjustments involved in a record high-jump, it is striking that all those events can occur within a time of less than one second. All the characteristics of the proprioceptive system must be evoked in their highest degree in carrying out such a skilled, rapid and yet violent movement.

Long-Jumping

It is well known to athletes that success in long-jumping consists in learning to jump high. It is not, of course, the case that a record long-jumper performs at the same moment a record high-jump. He must, however, cover a very considerable height. The world's record long-jump is 25.48 feet. With the check provided by the vertical impulse in the last step we can not well imagine the horizontal velocity to be greater, at this moment, than that of 100 yards completed in 10 seconds; that is, than 30 feet per second. Let us assume this value: then the performer remains in the air for $\frac{25.48}{30}$; that is, 0.85 second: hence we may calculate that the vertical distance covered is about 2.9 feet. Assuming the center of gravity of the subject to have been originally 3 feet high, this means that it must have reached a height 5.9 feet in the air, enough, in a high-jump, to enable its owner to clear 5.9 feet. It is interesting to find that the simple laws of mechanics emphasize so strongly

the precepts of the athletic trainer. Not only must one jump high if one wishes to break a long-jump record, but one must bring one's center of gravity nearly six feet high into the air; for one must project oneself vertically, so that one may remain for 0.85 second above the ground.

Conclusion

The practice of athletics is both a science and an art, and, just as art and science are the most potent ties tending to draw men together in a world of industrial competition, so sport and athletics, by urging men to friendly rivalry, may help to avert the bitterness resulting from less peaceful struggles. If, therefore, physiology can aid in the development of athletics as a science and an art, I think it will deserve well of mankind. As in all these things, however, the reward will be reciprocal. Obviously in the data of athletic records we have a store of information available for physiological study. Apart from its usefulness, however, I would urge that the study is amusing. Most people are interested, at any rate in England and America, in some type of sport. If they can be made to find it more interesting, as I have found it, by a scientific contemplation of the things which every sportsman knows, then that extra interest is its own defense.

SHELL-MOUNDS AND CHANGES IN THE SHELLS COMPOSING THEM

By EDWARD S. MORSE
SALEM, MASSACHUSETTS

The refuse piles of early races are scattered along the coast line of every continent. They are mainly composed of the shells of edible mollusks. Little attention was paid to them until Professor Steenstrup, the eminent Danish naturalist, was led to examine a number of these deposits along the shore of the Baltic Sea. These masses of shells along the shore lines had been regarded as natural configurations due to the elevation of the land and had been known in various parts of the world as upraised beaches. Steenstrup soon discovered that the shells composing these deposits were full grown, or nearly so; he also observed that the mass consisted of shells of four edible species and that these mollusks did not live in the same habitat or stretch of shore, some like the oyster living on rockbeds, others in sand. By this study the artificial character of the shell-heaps was established. Sir John Lubbock visited the Baltic shell-heaps with Steenstrup and in his classical work, "Prehistoric Times," he devotes a chapter to the subject, and in this chapter the student will find a most lucid account of Steenstrup's work.

The exploration of shell-heaps in various parts of the world has brought to light many objects of human workmanship, and though these deposits are literally the refuse piles or kitchen refuse of a rude and savage people, and most of the objects are in a fragmentary condition they have, nevertheless, thrown much light on the culture of the people who made them. A study of the bones and charred wood indicates a great change in the fauna and flora of the region since the deposits were made. A study of the shells composing these deposits shows that a change has taken place in the relative abundance of individuals of certain species, and in their relative size, relative proportions and in the extinction of certain species.

In a rather extensive study of the shell-mounds of Omori, near Tokyo, which I was first to recognize as an artificial deposit, and upon which I prepared a memoir which was published by the Imperial University, I found a marked variation in the amount of proportional change in the different species. A marked change was also seen in the relative abundance of certain species. One species

of shell, rare in the mounds, is common along the shores of Yedo Bay; three species common in the mounds are not found in Yedo Bay; two species common in the mounds are rare to-day; seven species are equally common in the mounds and living along the shores; seven species common in the Tokyo market are not found in the mounds and one species, *Arca granosa,* is not found living within five hundred miles of the mounds.

In the New England shell-heaps the common Pecten, or scallop, is found as far north as Maine, yet it is a southern shell and does not live north of Cape Cod, except at the extremity of the cape, where it is very abundant and extends a little way round on the northern shore. *Venus mercenaria,* the quahog, is found living in a few localities north of Cape Cod; formerly it must have been common along the whole coast as nearly every shell-heap revealed it. At the time the shell-heaps were formed in Casco Bay, Maine, the region was covered wtih a hard-wood growth; not only is this shown by the character of the charcoal, but a close examination of the original ground surface, under a deposit of shells five feet in thickness, shows several species of minute land shells that live only in hard-wood growths. Now, and from the earliest historic times, pine and spruce have covered the land. In the Baltic shell-heaps oak was the prevailing tree of the forest. Since the earliest historic records in Denmark beech has been the prevailing tree. The great auk, a northern bird, has long been extinct, yet in the shell-heap times it was common along the eastern coast of America even as far south as Florida, as is shown by the bones of the bird often found in the heaps.

Steenstrup gives a list of seven species of shells found in the Baltic shell-heaps and says, "It is remarkable that they are well developed and larger than any found in the neighborhood." The mass of the deposits is composed of oyster shells and this shell has entirely disappeared from the Baltic; the common clam, *Mya arenaria,* found everywhere in the Baltic, was not found in the shell-heaps. I visited Professor Steenstrup to ascertain how I could reach the Baltic shell-heaps and he told me that they had all been dug over and the relics all collected. I told him I was interested only in getting the shells of the common clam, and to my amazement he said there were no shells of Mya in the deposit. Though the clam abounded in the Baltic the prehistoric people never ate them. It was the same in England; the clam had never been eaten, even in ancient times. It is a common shell there, and thousands of barrels are shipped to the Newfoundland fisheries for bait. We learned the epicurean delights of the clam from the North American Indians, to whom we are indebted for the divine gift, tobacco.

Professor Jeffries Wyman observed several changes in the shells of the Florida shell-heaps. He says, in his memoir of the subject, that the Ampullaria and Paludinae are much larger than those living in the immediate vicinity.

Clarence Bloomfield Moore, who has made extensive explorations of the shell-mounds of Florida, in the *American Naturalist* for 1892 (Nov., p. 192), describes shell-heaps on the St. Johns River. He says: "It will be noted that the aperture of the shell, in specimens from the mound, measures from one half to about one third the length of the shell; but in recent specimens, from the adjacent creek, it is in every case over one half the shell's length. No living specimens on record attain the size of the average shells of some of the mounds."

It has been shown that any change in the normal physical surroundings of an animal effects a change in the size or form of a creature in proportion to the intensity of conditions affecting it. Species vary greatly in their susceptibility to these conditions. In some species, as shown by Bateson in his studies of the upraised beaches of the Aral Sea, no changes are observable; while *Cardium edule* has been profoundly modified, *Dreissina polymorpha* and *Hydrobia ulvae* have shown no change. The changes in the proportionate diameters of many of the species of shells in the deposits lining the Gulf of Maine, Massachusetts Bay, Long Island Sound and the Bay of Yedo can not be due to changes in the chemical content of the water, for these bodies of water are too vast to be subjected to such influences as affected the Baltic Sea, the Aral Sea and the Champlain Valley in glacial times. Temperature alone must be regarded as the physical cause, as it will be shown that the proportionate diameters of Mya and Natica vary north and south of Cape Cod where the waters vary greatly in temperature. In the glacial clays of Maine, Mya has an index of 66 correlated with the lower temperature of glacial waters. Sir William Dawson, in the Canadian Record of Science, 1889, records the finding of the long variety of *Mya truncata* in shallow and warm water habitats, while in the colder waters the short variety occurs.

The shell-heaps at Damariscotta, Maine, composed almost entirely of oyster shells, are among the largest on the coast[1] and one has to go to Florida to find any of equal size. The greatest height of the deposit at Damariscotta was thirty-five feet. The early shell-heaps along the coast of New England, especially north of Cape Cod, were made up of the shells of the common clam, *Mya arenaria*, and these deposits rarely exceed five feet in thickness. A very interesting feature of the Damariscotta deposits is the indisputable evidences of great age. This is determined by measuring the shells

[1] Removed for agricultural purposes.

of Mya, which are scattered through the deposits from the upper layers through to the very bottom layers. The results of these measurements proved that a change had taken place in their proportionate diameters during the growth of the deposits, and the earlier shells had a higher index than those found in the upper layers. So marked was the difference that the workmen engaged in removing the mass of shells averred that they had noticed that the clams in the base of the deposits were not only smaller but rounder.

In collecting the shells of Mya from the shell-heaps it is noticed that many are deformed by injuries, the result of the ruder methods employed by the Indians in using a pointed stick in prying them up from the mud. The clam digger to-day uses a four-pronged rake and pulls up a mass of clay from which he disengages the clams and thus avoids to a great extent the breaking of the shell. In measuring the clam I selected only those that were full grown, and of those only the toothless or right valves; furthermore, in choosing those for measurement, I selected only those shells that had perfect anterior and posterior margins.

I had a table extending the entire length of a thirty-foot room, along the edge of which I inserted measuring sticks having marked on them a scale of millimeters. The face of this measuring surface was on a level with the table, the shells were laid along the table, first longitudinally, the anterior and posterior edges of these shells just touching, the shells so placed that the ventral edge of one shell was in line with the dorsal side of the next one. Fifty or sixty shells were measured at one time. The point was to ascertain the proportional diameters: assuming the length of the shell to be 100, what was the height, that is, from the ventral edge of the shell to the beak? This was called the index. Now in every instance the index was higher in the shell-heap Mya than in the recent forms. The Mya in the shell-mounds of Omori, Japan, varied in precisely the same direction.

In a very interesting memoir, by William Bateson (Philo. Trans. 1889), on "Some variations of *Cardium edule,* apparently correlated to the conditions of life," he has shown some profound modifications which the shell has undergone in the drying up of certain lake regions around the Aral Sea with corresponding changes to be found in a series of terraces which, so to speak, formed a chronological sequence of beds. Among other interesting facts brought out by Mr. Bateson was the evidence already mentioned that while these changing conditions were correlated with the modifications of the shells of *Cardium edule* no appreciable change was observed in the shells of *Dreissina polymorpha* and *Hydrobia ulvae.* The physical changes taking place were an increased salinity of the

water and the shoaling of the water areas and a rise and greater variation of temperature. These changed physical conditions were correlated with the lengthening of the shell as compared to its height, a diminution in size and a reduction in the number of ribs.

In measuring the coiled shells, like Natica, the length and the breadth of the shell are indicated by the lines A, B and C.D in Fig. 1. A surface marked by closely ruled parallel lines will aid in adjusting the shell for measurement. Select only those shells that have the tip of the apex and base of the aperture unbroken, and then arrange them in the following manner: first ascertain the length (Fig. 2) and then their breadth as in Fig. 3. The eye is used in adjusting the shells for measurement and hence it is easier to have the closely ruled lines so that the shells can be more easily aligned. The difference in proportionate diameters are so great in most of the genera that this rough way of measurement is sufficiently accurate for the purpose.

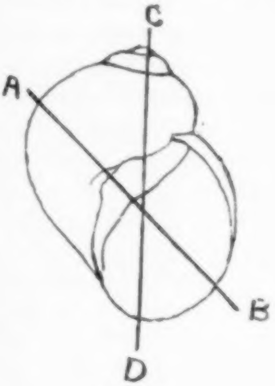


FIG. 1

I have collected my material from various shell-heaps along the shores of Maine, Massachusetts, Rhode Island and Connecticut. The Indians who made these deposits always established their camps in close proximity to mud-flats in which clams abounded, and usually where springs of water were found. It was assumed that the shells composing the shell-heaps were the ancestors of the clams

FIG. 2

FIG. 3

living in the immediate vicinity. In collecting the shells from the shell-heaps I made a similar collection of the living clams along the

adjacent shore. While the average index of the recent clam was 61.23, the shell heap clam was 63.37,[2] and if the index was higher in the ancient shell the recent shell on the neighboring shore in every case was also higher. As an example, the index of the clam in a shell-heap on Clapboard Island, Casco Bay, Maine, was extremely high, being nearly 64; in the living clam in the immediate vicinity the index was also high, being nearly 63; at Bar Harbor, Maine, the index of the shell-heap clam was 65, while the recent clam in the immediate vicinity was 63—striking evidences that the ancient clam was the direct ancestor of the clam living in the

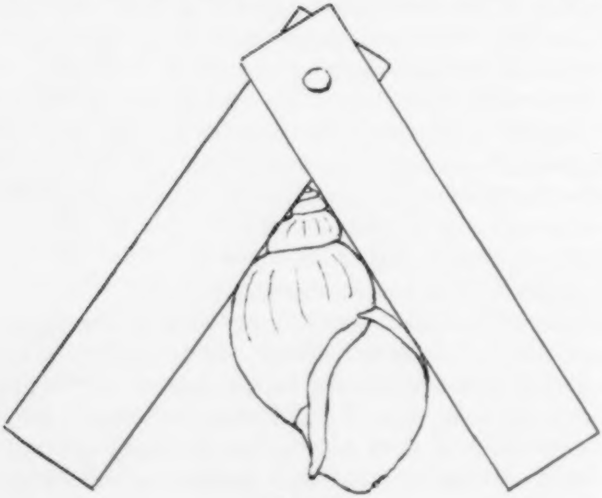


Fig. 4

vicinity to-day. The index of the recent clam north of Cape Cod is higher than the recent clam south of Cape Cod. The temperature of the waters south of Cape Cod is much higher than that of the waters north of the Cape. If then the index is correlated with temperature, the Mya in glacial clays should have a very high index; and this I found to be so in measuring shells of Mya from the glacial clays in Maine, specimens from glacial clays in the museum at Upsala, Sweden, and at Brighton, England, the index was 65 and 66.

[2] From twelve shell-mounds along the coast from Penobscot Bay, Maine, to Marblehead, Massachusetts, I measured 904 specimens of the common clam, *Mya arenaria*, and the average index was 63.37; from the adjacent shores of these mounds I got 976 specimens and the average index of these was 61.23. These were north of Cape Cod; south of Cape Cod I got from one station only seven perfect specimens, altogether too small a number to estimate upon. The average index, however, was 61.21, and of 302 recent clams, south of Cape Cod, the average index was 60.20.

Lunatia heros was not very common in the Marblehead shell-heap, which was a very small deposit. I managed to get thirty perfect specimens, and on the shore I picked thirty shells at random. The following list in millimeters is the measurements of these shells in the manner illustrated in Fig. 1. These figures run from the largest to the smallest shells collected.

In measuring coiled shells to ascertain the angle of the spire a simple device can be made of two strips of brass, or zinc, held together at one end by a rivet so that it can be opened or closed (Fig. 4). This device can be pushed down over the spire and then placed on the paper and the angle traced. With this I measured a number of species, and in every case the angle was less acute than in the recent form. Fig. 5 shows the angle of *Eburna japonica*, a common shell in the Omori shell-mounds and equally

Lunatia heros. PINE GROVE, MARBLEHEAD, MASSACHUSETTS

Recent		Ancient	
Height	Breadth	Height	Breadth
51	49	48	44
51	49	48	43
39	39	48	43
39	39	46	41
39	38	45	41
38	38	44	40
38	38	43	39
37	37	43	39
37	37	42	39
36	36	42	38
35	35	42	37
35	34	41	37
34	34	41	37
34	34	41	36
34	34	40	36
34	34	40	36
34	33	40	36
33	33	40	36
33	32	40	35
33	32	40	35
32	32	39	35
32	31	39	35
32	30	39	35
31	30	38	34
31	28	38	34
29	28	37	33
29	27	37	33
28	27	36	33
28	26	36	33
27	25	36	33

common along the adjacent shore. These angles represent the mean angle of hundreds measured; the inside line shows the angle of the recent form and the outside angle the ancient. In the New England shell-heaps the nearest relative of Eburna is *Buccinum undatum,* the shell shown in Fig. 4. It is very scarce in the mounds. Yet in the few measured the index was higher. On one island near Mount Desert, Maine, in a shell-heap I found 25 specimens and collected the same number on the beach, hardly enough to measure, yet the result was the same; the ancient shell had the less acute spire. The largest convoluted shells in New England are two species of Busycon, six or seven inches in length. These have never been found north of Cape Cod. The few I was able to get in a shell-heap south of Cape Cod distinctly showed a flatter spire.

A little black shell with eroded apex, *Nassa obsoleta,* is found living exclusively on mud-flats. It is also found in the shell-heaps and in such abundance sometimes that they must have been used for food. In an ancient deposit at Narragansett Pier, Rhode Island, I collected 284 specimens of this shell and on the adjacent mud-flats I collected an equal number, measuring them in the manner already described. I found the index in the recent shell was 55.8, while in the ancient form it was 56.7. In the Marblehead, Massachusetts, shell-heap I got 245 specimens of this shell, and on the mud-flats I collected 181 specimens; the index in the recent shell was 57.1 and in the ancient shell 58.5. The higher the index the less acute is the angle of the spire. The difference is slight, yet in both cases the recent shell has the more acute spire. The apex was eroded in the ancient and recent shell; in the Marblehead specimens the shells varied greatly in form; 134 were quite smooth, 49 were excessively eroded, and 62 had heavy folds. The ancient shell was longer, the average length being 21.50, while the recent form was 18.

FIG. 5

The large whelk, Buccinum, is rare in the mounds. On one island near Mount Desert, Maine, in a shell-heap I found 25 specimens and collected the same number on the beach, hardly enough to measure, yet the result was the same; the ancient shell had the less acute spire.

The changes in size and shape of the species of shells in the Omori deposits were so marked in contrast with the recent shells that they could be easily recognized along the beach where they had

been washed from the mounds. The different species varied in the size and proportion of their changes. In some the ancient forms were larger than the recent. In others they were smaller; in some species the changes were profound. Thus in the three species of Arca the ribs have increased in number.

	Ancient	Recent
Arca-sub-crenata	30	33
" -inflata	39	41
" -granosa	20	26

In *Arca inflata* the hinge area, or deck, in a single valve is 15 mm wide in the ancient form, while in the recent form it has been reduced to 5 mm. In the coiled shells the difference consisted in a more obtuse spire in the canaliculated forms in the shell-heap, while in the recent forms the spire was more acute.

It is interesting to remark that the *Purpura luteostoma* and *P. clavigera* are easily distinguished from one another in the recent forms collected at Enoshima and other places along the coast. In the shell-heaps the distinctions are not so easily made out, and it would seem that *Purpura perone, P. luteostoma, P. clavigera, P. tumulosa* and others were modifications of a single form.

In the Omori shell-mounds *Natica lamarckiana* was so different from the recent form that it might be regarded as a marked variety. The shell was equally abundant in the shell-heap and along the shore. In the ancient form the spire is much higher than in the recent form (Fig. 6). In the shell-heaps of New England a closely allied species, *Natica duplicata,* shows precisely the same change, as does also *Natica heros*. Fig. 7 is a photograph of eight specimens of *Natica heros;* the four on the left are from a shell-heap near Salem, the other four picked up from an adjacent beach, the ancient form having a much higher spire. I collected thirty-four specimens and picked up at random from the adjacent beach the same number and measured them. It will be seen that the length of the shell-heap Natica is much longer compared to its width than in the recent shell.

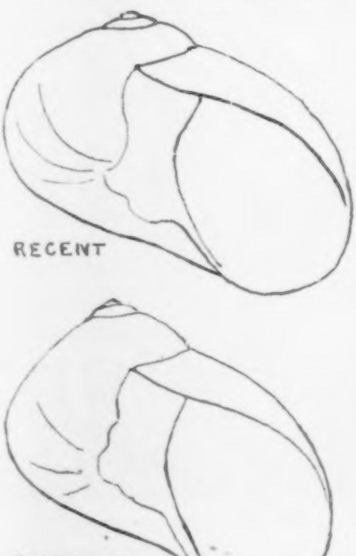
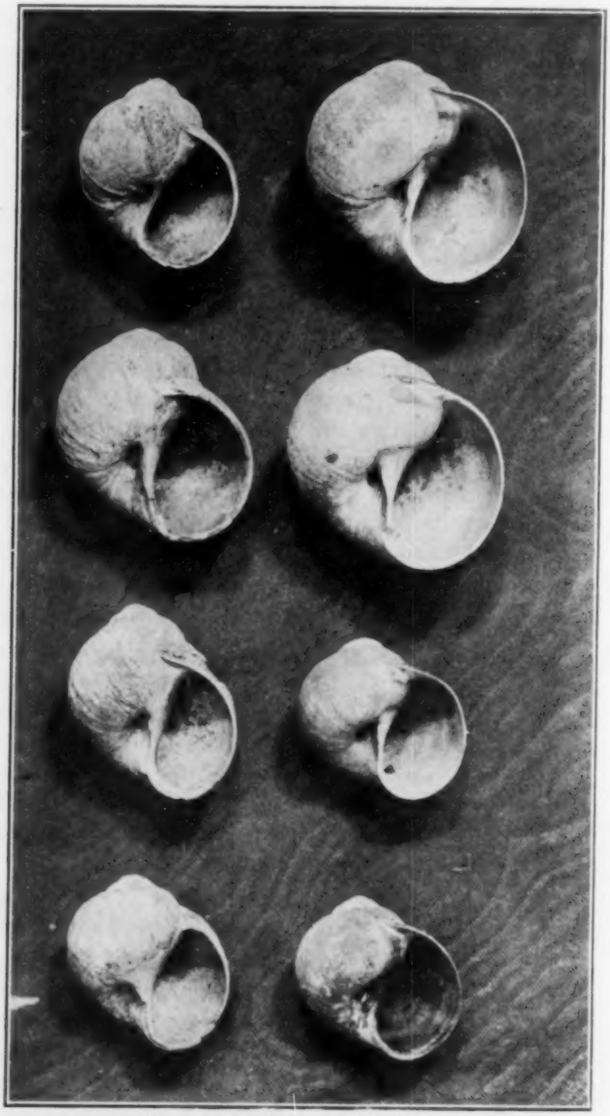


Fig. 6

The question of size alone would indicate matters of nutrition and the salinity of the water, and these variations might occur in a few generations. Changes in proportionate diameters imply a longer



Ancient FIG. 7 Modern

period. The change in this direction is a change toward specific distinctions. This is well shown in a study of the tertiary shells. In the lower tertiary all the species, or nearly all, are extinct; in the middle tertiary from 40 to 60 per cent. are extinct, in the upper

tertiary none, or but few, are extinct and in recent times the process goes on in a change in the proportionate diameters of the shell.

In an exceedingly interesting memoir, entitled "The Champlain Sea," by Miss Winifred Goldring,[3] the author has made an exhaustive study of the Pleistocene shells along the banks of the St. Lawrence and Champlain Valley, and has brought out the fact that going southward there is evidence of a marked change in the Pleistocene fauna similar to that seen in the living fauna of the Baltic Sea to-day. "A study of this fauna and comparison with the conditions found in the Baltic Sea and elsewhere, has led to the conclusion that the character of the post-glacial marine fauna is due in large part, at least, in the decreased salinity in this direction in the waters of that time. . . . The normal salt composition of sea water permits the development of a fauna rich in species and genera. A reduction of the salt content produces an impoverished fauna, poor in lime, dwarfed in size but often rich in individuals." In a graphic way the author has given the outlines of various genera, such as Macoma, Saxicava, Yoldia and others superimposed upon each other, showing the diminishing size of the specimens as the habitat receded from the normal salt content of the water to the southern region of the Champlain Valley and consequent freshening of the water. Not only does the freshening of the water dwarf the individual, but the proportionate diameters of some of the species have changed. Miss Goldring shows that "in *Yoldia actica,* from the Champlain area the modified form of the shell is very noticeable. . . . In the recent forms and those from Montreal and Ottawa areas there is a pronounced posterior extension or wing with subacute tip. . . . The extreme forms of the Champlain area are so different from the typical forms from the vicinity of Montreal and Ottawa that we will be inclined to regard them as belonging to another species."

In an interesting paper by Professor Henry W. Shimer, entitled "Dwarf faunas,"[4] an excellent résumé is given of many contributions on the subject. He says that physical conditions modified the character of the individual and all these species appeared dwarfed. The following are the chief agencies of dwarfing as noticed in recent and fossil faunas; a change in the normal chemical content of the water, such as freshening, concentrations of salt, iron, etc.; increase in certain gases, presence of mud and other mechanical impurities; variation in temperature, a floating habitat and extremes in depth of water. It seems curious that all these various influences

[3] New York State Museum Bull. 230, 240, seventeenth report of the director.

[4] *American Naturalist,* Vol. XLII, No. 499.

should modify the individual in one general way, namely, in dwarfing, in some instances causing deformities. William Bateson in his memoir entitled "On some variations of *Cardium edule*, apparently correlated to the conditions" (Philosophical Trans., 1889), says, "If by this examination any variation can be shown to occur regularly with the change of conditions or in any way in proportion to their intensity, it is so far evidence that there is a relation of cause and effect between them."

In my memoir on "The shell-mounds of Omori," I prepared a chapter entitled "A comparison between the ancient and modern molluscan fauna of Omori, Japan." Realizing that Charles Darwin would be interested in the evolutionary facts therein contained, I ventured to send him page proofs of the chapter with a plate figuring nine species, at the same time begging him not to acknowledge them. Nevertheless, I received the following letter which I can not refrain from publishing, as it has already appeared in the "Life and Letters of Charles Darwin."

> Although you are so kind as to tell me not to write I must just thank you for the proofs of your paper which has interested me greatly. The increase in the number of ridges in the three species of Arca seems to me a very noteworthy fact; as does the increase of size in so many yet not all the species. What a constant state of fluctuation the whole organic world seems to be in! It is interesting to hear that everywhere the first change apparently is in the proportional numbers of the species: I was much struck with this fact in the upraised shells at Coquinbo, in Chile, as mentioned in my geological observations in South America.
>
> Of all the wonders of the world the progress of Japan, in which you have been aiding, seems to me about the most wonderful.
>
> Believe me
> my dear Sir
> yours very truly
> Charles Darwin.

The changes we have seen in the proportionate diameters, relative size, abundance, etc., of the species of shells composing the shell-heaps in America and Japan indicate a vast lapse of time since the first deposits were made. Steenstrup believed that the Baltic shell-heaps belonged to the early stone age. The few stone implements found in the Omori, Japan, deposits were of the rudest character. The changes in the shape of the shells are an important illustration of the fact that when you have a unit of time sufficiently long species change, a significant fact for evolution.

THE PROGRESS OF SCIENCE

By Dr. EDWIN E. SLOSSON

SCIENCE SERVICE, WASHINGTON

AFTER A

FOR more than a dozen years chemists have been hunting for something that nobody has ever seen and yet everybody has to have. It is in our food; must be or else we starve with our stomach full. In lack of it, the white rat babies of the laboratory—and, what's worse, white human babies by the thousand—may die an early death or be stunted for life. Our sense of taste, which is generally a safe guide to nutritive values, fails us in the case of the vitamins, for we can not tell by the savor which foods contain these essential ingredients, yet if we fail to include such foods in our daily dietary we soon suffer for it in health and vigor.

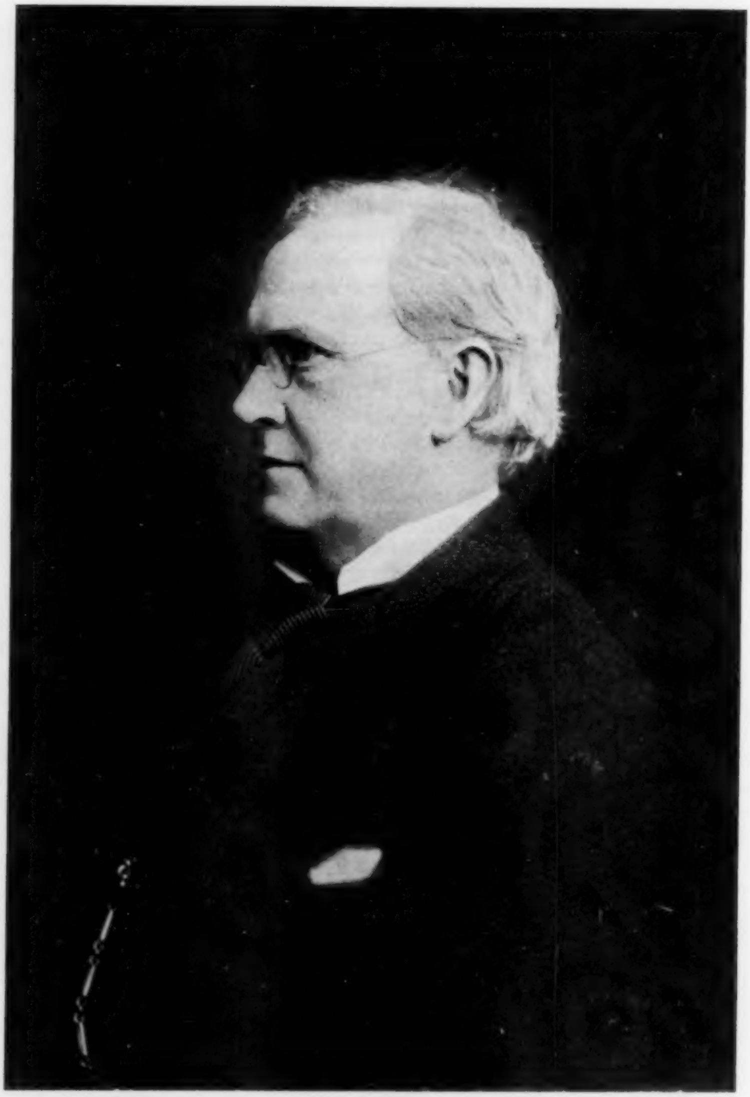
At the head of the list of vitamins is that known provisionally as "A." Chemists could not give it its proper name because they did not know what family of compounds it belonged to. They only knew that certain foods were short of something essential for growth and health.

Their problem was like the riddles that used to be popular with puzzlers. For instance: "My first is in butter, but not in lard." It is in sweet potatoes, but not in Irish. It is in yellow corn, but not in white. It is in codliver oil, but not in olive oil. What is it?

McCullom, of Madison, who found out these facts about 1912, called the evasive vitamin "unidentified dietary factor fat-soluble A," which expressed what was known at that time, but obviously was not the sort of snappy slogan that the advertiser of a breakfast food desires. Chemists all over the world have been trying ever since to isolate and purify Vitamin A, but unsuccessfully.

At last, however, the problem appears to have been solved and Japan may get the honor of isolating "A." Katsumi Takahashi and other investigators, working in the laboratory of Professor U. Suzuki in the Institute of Physical and Chemical Research at Tokyo, report having extracted and analyzed Vitamin A from codliver oil, spinach and green laver, a seaweed. It comes out finally as a yellowish red oil, transparent and viscous, with a characteristic but not disagreeable odor and a slightly bitter taste, resembling somewhat the yellow matter of carrots and green leaves. It is not so unstable as had been supposed, for it can be distilled in a vacuum without decomposition.

The courtesy of chemistry gives to the discoverer of a compound, like the father of a child, the right to christen it, and fortunately for the rest of the world, the Japanese chemists have not insisted upon giving their find a Japanese name. They have instead called it "biosterin" because it resembles in composition and behavior the already known "cholesterin," which occurs commonly in plant and animal cells, although its function is still a mystery.



DR. HORACE LAMB

PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, FOR THIRTY-FIVE YEARS PROFESSOR OF MATHEMATICS IN THE UNIVERSITY OF MANCHESTER.

One of the interesting peculiarities of biosterin is that it will print its image on a photographic plate in the dark. That is, it acts like radium in giving off some sort of active rays or emanation capable of producing an impression on the sensitive plate as light does. Various oils and terpenes will act like this, but none of them are so active.

The effect of a minute amount of biosterin on the vital processes is most amazing. A daily dose of no more than a millionth of a gram was sufficient to keep up the growth of young rats that were fed on a diet so deficient in this vitamin that they would otherwise stop growing and die. But, on the other hand, rats that took a drop too much died of it, like those who had none. The fatal dose is about ten thousand times the normal ration, so there is ample margin and no one is endangered by getting an overdose of biosterin in his food.

If this turns out to be really the long sought vitamin, it will mark the beginning of a new era in food science for chemists. When the chemist gets hold of a definite compound, he may make it in quantity, or others similar to it, which may have different effects. To be able to alter the nutritive value and influence of a diet by adding a drop or two of something puts into the hands of the chemist a new power of controlling the processes of life that may lead to strange results.

WAKING UP THE DEAD SEA

Among the many schemes for the development of Palestine one of the most original and ambitious is that for utilizing the Dead Sea as a source of water power.

This seems at first sight a startling suggestion. We are used to getting water power from mountain streams and lakes, but the Dead Sea is about 1,300 feet below the ocean level to start with.

But on second thought, we see that the scheme is not theoretically impossible, for if we can get power from water running down to the ocean, we can likewise get power from water running down from the ocean—provided that we can find a lower place to put it in. Even if we could find a sink at low level in which to run the waste water there would have to be some pumping arrangement to lift out the water as fast as it runs in, and this would require more power than could be got out of the water wheel.

Now the Dead Sea forms just such a sink as is needed and an adequate pump was long ago installed by providence and is already in operation, being supplied with power by the central station of the solar system. The sun sucks up the river Jordan as rapidly as it runs in and the engineers calculate that if as much water as this or more were siphoned in from the Mediterranean, it would be continuously evaporated from the expanded surface of the sea and the soaked sands of its shore. This is expected to provide over 600,000 horse-power for the electrification of the Holy Land.

The French Academy of Sciences, before which this scheme was presented, considered also the power possibilities of the other sub-sea sinks of the world, especially the Salton Sea, the Caspian Sea and certain sections of the Sahara.

The Salton Sea was formed or rather refilled about twenty years ago by flooding from the Imperial irrigation canal and the Alamo and New Rivers and it has been slowly drying up ever since. The surface is 206 feet below the Gulf of California. In 1917 its area was 300 square miles.

International News Reel Photos

DR. CHARLES W. ELIOT

PRESIDENT EMERITUS OF HARVARD UNIVERSITY, POSING FOR A PORTRAIT BUST BEING EXECUTED BY MR. C. S. PAOLO, OF NEW YORK. ON THE OCCASION OF DR. ELIOT'S NINETIETH BIRTHDAY LAST YEAR GREETINGS WERE SENT FROM A LARGE NUMBER OF SOCIETIES AND OTHER ORGANIZATIONS, AND THESE HAVE BEEN PUBLISHED IN A COMMEMORATIVE VOLUME.

It is, therefore, about as large as the Dead Sea but only a sixth as deep below sea-level. The evaporation rate at Salton Sea is about half that of the Dead Sea, so the total theoretical horse-power obtainable by running into the Salton Sea from the Gulf of California, ninety miles distant, all the water that can be evaporated away would not produce over 35,000 horse-power. But it is useless to talk about the project anyhow, for the Californians would lynch any one who proposed to turn the Salton Sea into a salt sea permanently, when it could be better used as farming land. They are determined that no more water shall be run into their sink.

The idea of making a sea out of the Sahara was much discussed in the last century, not for the purposes of power, but to open up the heart of Africa to navigation, make a seaport out of Timbuctoo, and ameliorate the climate. It was argued that it was only necessary to cut through a narrow rim of north Africa and let in the waters of the Mediterranean, which would form there a second Mediterranean, surrounded by fertile shores and flourishing cities. The British protested that flooding the Sahara would divert the Gulf Stream into the Straits of Gibraltar and leave England as cold as Labrador.

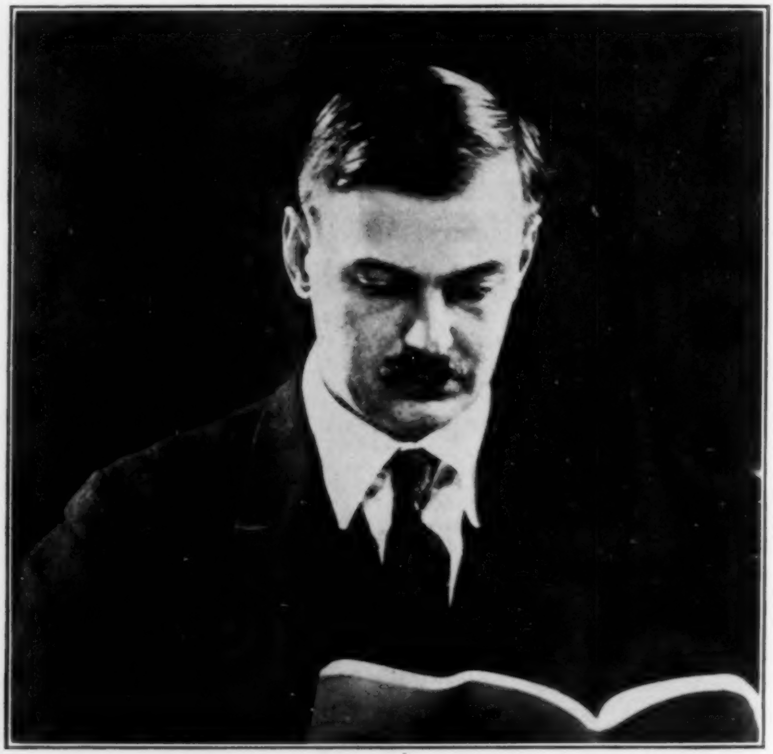
But both the hopes and the fears vanished when some one took the trouble to look at a topographic map of Africa and observed that the average altitude of land proposed to be submerged was over a thousand feet. Only a very small portion of the Sahara is below sea level; certain salt marshes in southern Tunis and half a dozen oases in eastern Libya, and these were only from fifty to a hundred feet below the Mediterranean.

So the great project for the navigation of the Sahara collapsed and is now principally remembered because it afforded Ibsen a theme for one of Peer Gynt's chimerical schemes. This is his vision as a penniless castaway in Morocco:

> "The sea's to the west; it lies piled up behind me,
> Dammed out from the desert by a sloping ridge."
>
> "Dammed out? It wants but a gap, a canal,—
> Like a flood of life would the waters rush
> In through the channel, and fill the desert!
> Soon would the whole of yon red-hot grave
> Spread forth, a breezy and rippling sea.
> The oases would rise in the midst, like islands;
> Atlas would tower in green cliffs on the north:
> Sailing ships would, like stray birds on the wing,
> Skim to the south, on the caravans' track."
>
> "The southland, behind the Sahara's wall,
> Would make a new seaboard for civilization.
> Steam would set Timbuctoo's factories spinning."
>
> "Skirting a bay, on a shelving strand,
> I'll build the chief city, Peeropolis.
> The world is decrepit! Now comes the turn
> Of Gyntiana, my virgin land!"

A PAYING GUEST

A NEW and startling theory of how we got our good red blood is advanced by Mr. Needham, of Cambridge. He suggests that the red corpuscles, now a necessary factor in animal life, first entered as foreign invaders in search of food. Sometime back in the Pre-Cambrian, he surmises, when the ancestors of all mammals were still swimimng in the sea and had not yet closed their circulatory system, they were penetrated by certain single and free-swimming cells,



DR. CLARENCE C. LITTLE

ELECTED PRESIDENT OF THE UNIVERSITY OF MICHIGAN, WHILE OCCUPYING THE PRESIDENCY OF THE UNIVERSITY OF MAINE. PREVIOUSLY DR. LITTLE WAS ASSISTANT DIRECTOR OF THE STATION FOR EXPERIMENTAL EVOLUTION OF THE CARNEGIE INSTITUTION WHERE HE CARRIED ON IMPORTANT WORK IN GENETICS

which, finding here abundance of nitrogenous nutriment, made themselves at home and in time became indispensable to their host. They swallowed the red coloring matter, a waste product which had been hard to get rid of, and used this as a medium for carrying fresh oxygen from the lungs to the muscles, so when the creature took to living on land it was able to make full use of the free air it found there.

Many such cases of partnership for mutual benefit are known to biologists, who call the arrangement "symbiosis." Certain sea-worms operate a system closely corresponding to this hypothetical scheme. Being devoid of chlorophyll, the green coloring matter of plants, they have no way of manufacturing sugary foods for themselves. But after they are infected with the small green cells of certain algae the needs of both are satisfied. The green guests prepare carbohydrates by aid of the sunshine and in turn live on the protein products of their hosts.

But if the green plant cells fail to keep up the food supply the animal gets hungry and digests the vegetable invaders, although this means sui-

cide. Something of this sort happens in the animal body, when the red blood corpuscles dissolve and disappear faster than they can be replaced, "pernicious anemia" the doctors call it. But the person who shows such ingratitude to the uninvited guests that have become such useful servants is sure to suffer for it.

BLOOD RELATIONS IN PLANT FAMILIES

Plants have no blood, yet a German botanist has found it possible to use their juices to determine their real relationships just as comparative tests on the blood of animals show which are nearest of kin. He has shown by this method of serum diagnosis that, for example, the common milkwort displays affinity with the heather, bittersweet and horse-chestnut families; the bear-berry with the heather, bittersweet, milkwort and grape families.

What the test actually shows is merely that the proteins of these plants are similar in composition, but from this an actual family kinship, coming from a common ancestry, may be reasonably inferred. Hitherto, botanists have had no way of ascertaining the family connections of plants, and so they have classified plants according to their external forms and features, such as the number of the petals, the shape of the leaves, and the like. But this is an uncertain system since plants of recent species may develop close resemblances in appearance and structure when grown under similar climatic conditions. The new chemical method of classification by composition is likely to lead to safer conclusions.

Using the serum test on animals it has been found that the blood of man corresponds more closely to that of the large tailless apes of the Old World than to the smaller tailed monkeys of the New World, while the blood of other animals differs decidedly from human blood.

PICTURE TELEGRAPHY

Whenever an author writes a romance of Utopian life some centuries in the future he introduces as one of the marvelous inventions of that period an instrument for seeing what is going on at a distance. Usually it is modeled after the telephone with a disk in which one can see mirrored the scene at the other end of the wire. I do not remember that any of these novelists of the twenty-first century and after have dared to discard the wire, which shows how difficult it is nowadays for the imagination to get ahead of the facts. Already we hear that wireless pictures and wireless movies will be added to the wireless telephone.

But even though long-distance photography is slow to enter into broadcasting, it will be a great thing for illustrated journalism. News comes now by wire and the pictures follow by slow freight, arriving usually a week or so after people have lost interest in the event. This delay places too much of a strain on the editor's conscience. He is sometimes unable to resist the temptation to put a stock cut to a new use or to touch up a photograph.

When Father Gapon led his procession to their death in St. Petersburg on Bloody Sunday in the first Russian revolution the American papers came out with half a dozen different portraits of him, all typical Russian revolutionists; any one of them might have looked like him, but un-

Henry Miller's News Picture Service, Inc.

DR. CHARLES CHROE

WHO WILL RETIRE SHORTLY AFTER THIRTY-TWO YEARS SERVICE AS CURATOR OF THE KEW OBSERVATORY, LONDON

fortunately none of them did. When San Francisco was burning, the most enterprising of the New York papers published a photograph of the city in flames with very natural looking smoke rolling up from it. Unfortunately the staff artist who adapted it neglected to erase the date of the copyright, which was several years before the catastrophe. During the great war the press photographers were able to produce from their stock rooms the portrait of any general whose name was cabled over. We saw pictures of the tanks and Fokkers as soon as we heard of them, though these did not look much like those that appeared later. Doubtless the early designs were abandoned.

Such accidents have a tendency to impair the implicity of the confidence which the dear reader should have in his favorite periodical. Besides, the moral character and future prospects of an editor deserve consideration. But perhaps this new machine, like all the others, will bring with it more powerful and insidious temptations. "God made man upright, but they have sought out many inventions."